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Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Control Analysis

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This article uses more complete state panel data (through 2014) and new statistical techniques to estimate the impact on violent crime when states adopt right-to-carry (RTC) concealed handgun laws. Our preferred panel data regression specification, unlike the statistical model of Lott and Mustard that had previously been offered as evidence of crime-reducing RTC laws, both satisfies the parallel trends assumption and generates statistically significant estimates showing RTC laws *increase* overall violent crime. Our synthetic control approach also finds that RTC laws are associated with 13–15 percent *higher* aggregate violent crime rates 10 years after adoption. Using a consensus estimate of the elasticity of crime with respect to incarceration of 0.15, the average RTC state would need to roughly double its prison population to offset the increase in violent crime caused by RTC adoption.

I. Introduction

For two decades, there has been a spirited academic debate over whether "shall-issue" concealed carry laws (also known as right-to-carry or RTC laws) have an important impact on crime. The "More Guns, Less Crime" hypothesis originally articulated by John Lott and David Mustard (1997) claimed that RTC laws decreased violent

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crime (possibly shifting criminals in the direction of committing more property crime to avoid armed citizens). This research may well have encouraged state legislatures to adopt RTC laws, arguably making the pair's 1997 paper in the *Journal of Legal Studies* one of the most consequential criminological articles published in the last 25 years.

The original Lott and Mustard paper as well as subsequent work by John Lott in his 1998 book *More Guns, Less Crime* used a panel data analysis to support the theory that RTC laws reduce violent crime. A large number of papers examined the Lott thesis, with decidedly mixed results. An array of studies, primarily those using the limited data initially employed by Lott and Mustard for the period 1977–1992 and those failing to adjust their standard errors by clustering, supported the Lott and Mustard thesis, while a host of other papers were skeptical of the Lott findings.[1]

It was hoped that the 2005 National Research Council report *Firearms and Violence: A Critical Review* (hereafter the NRC Report) would resolve the controversy over the impact of RTC laws, but this was not to be. While one member of the committee—James Q. Wilson—did partially endorse the Lott thesis by saying there was evidence that murders fell when RTC laws were adopted, the other 15 members of the panel pointedly criticized Wilson's claim, saying that "the scientific evidence does not support his position." The majority emphasized that the estimated effects of RTC laws were highly sensitive to the particular choice of explanatory variables and thus concluded that the panel data evidence through 2000 was too fragile to support any conclusion about the true effects of these laws.

This article answers the call of the NRC Report for more and better data and new statistical techniques to be brought to bear on the issue of the impact of RTC laws on crime. First, we revisit the state panel data evidence to see if extending the data for an additional 14 years, thereby providing additional crime data for prior RTC states as well as on 11 newly adopting RTC states, offers any clearer picture of the causal impact of allowing citizens to carry concealed weapons. We distill from an array of different panel data regressions for various crime categories for two time periods using two major sets of explanatory variables—including our preferred specification (DAW) and that of Lott and Mustard (LM)—a subset of regressions that satisfy the critical parallel trends assumption. All the statistically significant results from these regressions show RTC laws are associated with *higher* rates of overall violent crime, property crime, or murder.

Second, to address some of the weaknesses of panel data models, we undertake an extensive synthetic control analysis in order to present the most complete and robust

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[1]In support of Lott and Mustard (1997), see Lott's 1998 book *More Guns, Less Crime* (and the 2000 and 2010 editions). Ayres and Donohue (2003) and the 2005 National Research Council report *Firearms and Violence: A Critical Review* dismissed the Lott/Mustard hypothesis as lacking credible statistical support, as did Aneja et al. (2011) (and Aneja et al. (2014) further expanding the latter). Moody and Marvell (2008) and Moody et al. (2014) continued to argue in favor of a crime-reducing effect of RTC laws, although Zimmerman (2014) and McElroy and Wang (2017) find that RTC laws *increase* violent crime and Siegel et al. (2017) find RTC laws increase murders, as discussed in Section III.B.

results to guide policy in this area.[2] This synthetic control methodology—first introduced in Abadie and Gardeazabal (2003) and expanded in Abadie et al. (2010, 2014)—uses a matching methodology to create a credible "synthetic control" based on a weighted average of other states that best matches the prepassage pattern of crime for each "treated" state, which can then be used to estimate the likely path of crime if RTC-adopting states had not adopted an RTC law. By comparing the actual crime pattern for RTC-adopting states with the estimated synthetic controls in the postpassage period, we derive year-by-year estimates for the impact of RTC laws in the 10 years following adoption.[3]

To preview our major findings, the synthetic control estimate of the average impact of RTC laws across the 33 states that adopt between 1981 and 2007[4] indicates that violent crime is substantially higher after 10 years than would have been the case had the RTC law not been adopted. Essentially, for violent crime, the synthetic control approach provides a similar portrayal of RTC laws as that provided by the DAW panel data model and undermines the results of the LM panel data model. According to the aggregate synthetic control models—regardless of whether one uses the DAW or LM covariates—RTC laws led to increases in violent crime of 13–15 percent after 10 years, with positive but not statistically significant effects on property crime and murder. The median effect of RTC adoption after 10 years is 12.3 percent if one considers all 31 states with 10 years worth of data and 11.1 percent if one limits the analysis to the 26 states with the most compelling prepassage fit between the adopting states and their synthetic controls. Comparing our DAW specification findings with the results generated using placebo treatments, we are able to reject the null hypothesis that RTC laws have no impact on aggregate violent crime.

The structure of the article proceeds as follows. Section II begins with a discussion of the ways in which increased carrying of guns could either dampen crime (by thwarting or deterring criminals) or increase crime by directly facilitating violence or aggression by permit holders (or others), greatly expanding the loss and theft of guns, and burdening the functioning of the police in ways that diminish their effectiveness in controlling crime. We then show that a simple comparison of the drop in violent crime from

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[2]Abadie et al. (2014) identify a number of possible problems with panel regression techniques, including the danger of extrapolation when the observable characteristics of the treated area are outside the range of the corresponding characteristics for the other observations in the sample.

[3]The accuracy of this matching can be qualitatively assessed by examining the root mean square prediction error (RMSPE) of the synthetic control in the pretreatment period (or a variation on this RMSPE implemented in this article), and the statistical significance of the estimated treatment effect can be approximated by running a series of placebo estimates and examining the size of the estimated treatment effect in comparison to the distribution of placebo treatment effects.

[4]Note that we do not supply a synthetic control estimate for Indiana, even though it passed its RTC law in 1980, owing to the fact that we do not have enough pretreatment years to accurately match the state with an appropriate synthetic control. Including Indiana as a treatment state, though, would not meaningfully change our results. Similarly, we do not generate synthetic control estimates for Iowa and Wisconsin (whose RTC laws went into effect in 2011) or for Illinois (2014 RTC law), because of the limited postpassage data.

1977–2014 in the states that have resisted the adoption of RTC laws is almost an order of magnitude greater than in RTC-adopting states (a 42.3 percent drop vs. a 4.3 percent drop), although a spartan panel data model with only state and year effects reduces the differential to 20.2 percent. Section III discusses the panel data results, showing that the DAW model indicates that RTC laws have increased violent and property crime, with weaker evidence that RTC laws increased homicide (but not non-gun homicide) over our entire data period, while both the DAW and the LM model provide statistically significant evidence that RTC laws have increased murder in the postcrack period.

The remainder of the article shows that, using either the DAW or LM explanatory variables, the synthetic control approach uniformly supports the conclusion that RTC laws lead to substantial increases in violent crime. Section IV describes the details of our implementation of the synthetic control approach and shows that the mean and median estimates of the impact of RTC laws show greater than double-digit increases by the 10th year after adoption. Section V provides aggregate synthetic control estimates of the impact of RTC laws, and Section VI concludes.

II. The Impact of RTC Laws: Theoretical Considerations and Simple Comparisons

A. *Gun Carrying and Crime*

1. Mechanisms of Crime Reduction

Allowing citizens to carry concealed handguns can influence violent crime in a number of ways, some benign and some invidious. Violent crime can fall if criminals are deterred by the prospect of meeting armed resistance, and potential victims or armed bystanders may thwart or terminate attacks by either brandishing weapons or actually firing on the potential assailants. For example, in 2012, a Pennsylvania concealed carry permit holder became angry when he was asked to leave a bar because he was carrying a weapon and, in the ensuing argument, he shot two men, killing one, before another permit holder shot him (Kalinowski 2012). Two years later, a psychiatric patient in Pennsylvania killed his caseworker, and grazed his psychiatrist before the doctor shot back with his own gun, ending the assault by wounding the assailant (Associated Press 2014).

The impact of the Pennsylvania RTC law is somewhat ambiguous in both these cases. In the bar shooting, it was a permit holder who started the killing and another who ended it, so the RTC law may actually have increased crime. The case of the doctor's use of force is more clearly benign, although the RTC law may have made no difference: a doctor who routinely deals with violent and deranged patients would typically be able to secure a permit to carry a gun even under a may-issue regime. Only a statistical analysis can reveal whether in aggregate extending gun carrying beyond those with a demonstrated need and good character, as shall-issue laws do, imposes or reduces overall costs.

Some defensive gun uses can be socially costly and contentious even if they do avoid a robbery or an assault. For example, in 1984, when four teens accosted Bernie Goetz on a New York City subway, he prevented an anticipated robbery by shooting all four, permanently paralyzing one.[5] In 2010, a Pennsylvania concealed carry holder argued that he used a gun to thwart a beating. After a night out drinking, Gerald Ung, a 28-year-old Temple University law student, shot a 23-year-old former star lacrosse player from Villanova, Eddie DiDonato, when DiDonato rushed Ung angrily and aggressively after an altercation that began when DiDonato was bumped while doing chin ups on scaffolding on the street in Philadelphia. When prosecuted, Ung testified that he always carried his loaded gun when he went out drinking. A video of the incident shows that Ung was belligerent and had to be restrained by his friends before the dispute became more physical, which raises the question of whether his gun carrying contributed to his belligerence, and hence was a factor that precipitated the confrontation. Ung, who shot DiDonato six times, leaving DiDonato partially paralyzed with a bullet lodged in his spine, was acquitted of attempted murder, aggravated assault, and possessing an instrument of crime (Slobodzian 2011). While Ung avoided criminal liability and a possible beating, he was still prosecuted and then hit with a major civil action, and the incident did impose significant social costs, as shootings frequently do.[6]

In any event, the use of a gun by a concealed carry permit holder to thwart a crime is a statistically rare phenomenon. Even with the enormous stock of guns in the United States, the vast majority of the time that someone is threatened with violent crime no gun will be wielded defensively. A five-year study of such violent victimizations in the United States found that victims reported failing to defend or to threaten the criminal with a gun 99.2 percent of the time—this in a country with 300 million guns in civilian hands (Planty & Truman 2013). Adding 16 million permit holders who often dwell in low-crime areas may not yield many opportunities for effective defensive use for the roughly 1 percent of Americans who experience a violent crime in a given year, especially since criminals can attack in ways that preempt defensive measures.[7]

2. Mechanisms of Increasing Crime

Since the statistical evidence presented in this article suggests that the benign effects of RTC laws are outweighed by the harmful effects, we consider five ways in which RTC laws could increase crime: (a) elevated crime by RTC permit holders or by others, which can be induced by the greater belligerence of permit holders that can attend gun carrying or even through counterproductive attempts by permit holders to intervene protectively; (b) increased crime by those who acquire the guns of permit holders via loss or theft; (c) a change in culture induced by the hyper-vigilance about one's rights and the need

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[5]The injury to Darrell Cabey was so damaging that he remains confined to a wheelchair and functions with the intellect of an eight-year-old, for which he received a judgment of $43 million against Goetz, albeit without satisfaction (Biography.com 2016).

[6]According to the civil lawsuit brought by DiDonato, his injuries included "severe neurological impairment, inability to control his bowels, depression and severe neurologic injuries" (Lat 2012).

[7]Even big city police officers rarely need to fire a weapon despite their far greater exposure to criminals. According to a 2016 Pew Research Center survey of 7,917 sworn officers working in departments with 100 or more officers, "only about a quarter (27%) of all officers say they have ever fired their service weapon while on the job" (Morin & Mercer 2017).

to avenge wrongs that the gun culture can nurture; (d) elevated harm as criminals respond to the possibility of armed resistance by increasing their gun carrying and escalating their level of violence; and (e) all of the above factors will either take up police time or increase the risks the police face, thereby impairing the crime-fighting ability of police in ways that can increase crime.

a. Crime committed or induced by permit holders: RTC laws can lead to an increase in violent crime by increasing the likelihood a generally law-abiding citizen will commit a crime or increasing the criminal behavior of others. Moreover, RTC laws may facilitate the criminal conduct of those who generally have a criminal intent. We consider these two avenues below.

i. The pathway from the law-abiding citizen

Evidence from a nationally representative sample of 4,947 individuals indicates that Americans tend to overestimate their gun-related abilities. For example, 82.6 percent believed they were less likely than the average person to use a gun in anger. When asked about their "ability to responsibly own a handgun," 50 percent of the respondents deemed themselves to be in the top 10 percent and 23 percent placed their ability within the top 1 percent of the U.S. population. Such overconfidence has been found to increase risk taking and could well lead to an array of socially harmful consequences ranging from criminal misconduct and gun accidents to lost or stolen guns (Stark & Sachau 2016).

In a number of well-publicized cases, concealed carry permit holders have increased the homicide toll by killing someone with whom they became angry over an insignificant issue, ranging from merging on a highway and talking on a phone in a theater to playing loud music at a gas station (Lozano 2017; Levenson 2017; Scherer 2016). In one particularly tragic example in January 2019 at a bar in State College, Pennsylvania, a lawful permit holder, Jordan Witmer, got into a fight with his girlfriend. When a father and son sitting at the bar tried to intervene, Witmer killed both of them, shot his girlfriend in the chest, and fled. When his car crashed, Witmer broke into a nearby house, killed the 82-year-old homeowner, who was with his wife on their 60th wedding anniversary, and then killed himself (Sauro 2019). Another such example occurred in July 2018 when Michael Drejka started to hassle a woman sitting in a car in a disabled parking spot while her husband and five-year-old son ran into a store. When the husband emerged, he pushed Drejka to the ground, who then killed him with a shot to the chest. The killing is caught on video and Drejka is being prosecuted for manslaughter in Clearwater, Florida (Simon 2018).

When Philadelphia permit holder Louis Mockewich shot and killed a popular youth football coach (another permit holder carrying his gun) over a dispute concerning snow shoveling in January 2000, Mockewich's car had an NRA bumper sticker reading "Armed with Pride" (Gibbons & Moran 2000). An angry young man, with somewhat of a paranoid streak, who has not yet been convicted of a crime or adjudicated as a "mental defective," may be encouraged to carry a gun if he resides in an RTC state.[8] That such

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[8]The Gun Control Act of 1968 prohibits gun possession by felons and adjudicated "mental defectives" (18 U.S.C. 922(d)(4), 2016).

individuals will be more likely to be aggressive once armed and hence more likely to stimulate violence by others should not be surprising.

Recent evidence suggests that as gun carrying is increasing with the proliferation of RTC laws, road rage incidents involving guns are rising (Biette-Timmons 2017; Plumlee 2012). Incidents in which "someone in a car brandished a gun in a threatening manner or fired a gun at another driver or passenger have more than doubled in the last three years, from 247 in 2014 to 620 in 2016 …. The highest-profile recent road rage incidents involved two NFL players, Joe McKnight and Will Smith, killed … in separate road rage shootings in New Orleans" (Shen 2017).[9] In the nightmare case for RTC, two Michigan permit-holding drivers pulled over to battle over a tailgating dispute in September 2013 and each shot and killed the other (Stuart 2013). Without Michigan's RTC law, this would likely have not been a double homicide. Indeed, two studies—one for Arizona and one for the nation as a whole—found that "the evidence indicates that those with guns in the vehicle are more likely to engage in 'road rage'" (Hemenway et al. 2006; Miller et al. 2002).[10] These studies may suggest either that gun carrying emboldens more aggressive behavior or reflects a selection effect for more aggressive individuals.[11] If this is correct, then it may not be a coincidence that there are so many cases in which a concealed carry holder acts belligerently and is shot by another permit holder.[12]

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[9]Joe McNight and Ronald Gasser were arguing through their open car windows as they drove for miles. When they were both stopped at a red light, McNight walked over to Gasser's car, and the "two argued through the passenger-side window until Gasser pulled a gun from between his seat and the center console and shot McKnight three times." Gasser was convicted of manslaughter and sentenced to a prison term of 30 years (Calder 2018).

[10]A perfect illustration was provided by 25-year-old Minnesota concealed carry permit holder Alexander Weiss, who got into an argument after a fender bender caused by a 17-year-old driver. Since the police had been called, it is hard to imagine that this event could end tragically—unless someone had a gun. Unfortunately, Weiss, who had a bumper sticker on his car saying "Gun Control Means Hitting Your Target," killed the 17-year-old with one shot to the chest and has been charged with second-degree murder (KIMT 2018).

[11]While concealed carry permit holders should be free of any felony conviction, and thus show a lower overall rate of violence than a group that contains felons, a study in Texas found that when permit holders do commit a crime, it tends to be a severe one: "the concentration of convictions for weapons offenses, threatening someone with a firearm, and intentionally killing a person stem from the ready availability of a handgun for CHL holders" (Phillips et al. 2013). See, for example, a Texas permit holder who told police he shot a man in the head at an IHOP restaurant in Galveston because "he was annoyed by the noise the victim and others were making just a table away" (ABC News 2018).

[12]We have just cited three of them: the 2012 Pennsylvania bar shooting, the 2000 Philadelphia snow-shoveling dispute, and the 2013 Michigan road-rage incident. Here are two more. Former NFL player Will Smith, a concealed carry permit holder with a loaded gun in his car, was engaged in a road rage incident with another permit holder, who killed him with seven shots in the back and one into his side and shot his wife, hitting both knees. The shooter was convicted of manslaughter and sentenced to 25 years in prison (Lane 2018). In yet another recent case, two permit holders glowered at each other in a Chicago gas station, and when one drew his weapon, the second man pulled out his own gun and killed the 43-year-old instigator, who died in front of his son, daughter, and pregnant daughter-in-law (Hernandez 2017). A video of the encounter can be found at https://www.youtube.com/watch?v=I2j9vvDHlBU. According to the police report obtained by the *Chicago Tribune*, a bullet from the gun exchange broke the picture window of a nearby garden apartment and another shattered the window of a car with four occupants that was driving past the gas station. No charges were brought against the surviving permit holder, who shot first but in response to the threat initiated by the other permit holder.

In general, the critique that the relatively low number of permit revocations proves that permit holders do not commit enough crime to substantially elevate violent criminality is misguided for a variety of reasons. First, only a small fraction of 1 percent of Americans commits a gun crime each year, so we do not expect even a random group of Americans to commit much crime, let alone a group purged of convicted felons. Nonetheless, permit revocations clearly understate the criminal misconduct of permit holders, since not all violent criminals are caught and we have just seen five cases where six permit holders were killed, so no permit revocation or criminal prosecution would have occurred regardless of any criminality by the deceased.[13] Second, and perhaps more importantly, RTC laws increase crime by individuals other than permit holders in a variety of ways. The messages of the gun culture, perhaps reinforced by the adoption of RTC laws, can promote fear and anger, which are emotions that can invite more hostile confrontations leading to violence. For example, if permit holder George Zimmerman hassled Trayvon Martin only because Zimmerman was armed, then the presence of Zimmerman's gun could be deemed to have encouraged a hostile confrontation, regardless of who ultimately becomes violent.[14]

Even well-intentioned interventions by permit holders intending to stop a crime have elevated the crime count when they ended with the permit holder either being killed by the criminal[15] or shooting an innocent party by

[13]In addition, NRA-advocated state laws that ban the release of information about whether those arrested for even the most atrocious crimes are RTC permit holders make it extremely difficult to monitor their criminal conduct.

[14]Psychologists have found that the very act of carrying a gun tends to distort perceptions of reality in a way that exaggerates perceived threats. "We have shown here that … the act of wielding a firearm raises the likelihood that nonthreatening objects will be perceived as threats. This bias can clearly be horrific for victims of accidental shootings" (Witt & Brockmole 2012). As one permit holder explained: "a gun causes its bearer to see the world differently. A well-lit city sidewalk full of innocent pedestrians becomes a scene—a human grouping one of whose constituents you might need to shoot. Something good in yourself is, by this means, sacrificed. And more. In a sudden, unwieldy hauling-out of your piece, or just by having your piece in your pocket, you can fumble around and shoot yourself, as often happens and isn't at all funny. Or you might shoot some little girl on a porch across the street or two streets away, or five streets away. Lots and lots of untoward things can happen when you're legally carrying a concealed firearm. One or two of them might turn out to be beneficial—to you. But a majority are beneficial to neither man nor beast. Boats are said, by less nautical types, always to be seeking a place to sink. Guns—no matter who has them—are always seeking an opportunity to go off. Anybody who says different is a fool or a liar or both" (Ford 2016).

[15]In 2016 in Arlington, Texas, a man in a domestic dispute shot at a woman and then tried to drive off (under Texas law it was lawful for him to be carrying his gun in his car, even though he did not have a concealed carry permit.) When he was confronted by a permit holder, the shooter slapped the permit holder's gun out of his hand and then killed him with a shot to the head. Shortly thereafter, the shooter turned himself into the police (Mettler 2016). Similarly, when armed criminals entered a Las Vegas Walmart in 2014 and told everyone to get out because "[t]his is a revolution," one permit holder told his friend he would stay to confront the threat. He was gunned down shortly before the police arrived, adding to the death toll rather than reducing it (NBC News 2014). Finally, in January 2010, Stephen Sharp arrived at work at a St. Louis power plant just as co-worker Timothy Hendron began firing at fellow workers with an AK-47. Retrieving a pistol from his truck, Sharp opened fire at Hendron, and fecklessly discharged all six rounds from across the parking lot. Unharmed, Hendron returned fire, grievously wounding Sharp and continuing his rampage unabated. When the police arrived, there was "no clear distinction between attacker and victims." In the end, Hendron killed three and wounded five before killing himself (Byers 2010).

mistake.[16] Indeed, an FBI study of 160 active shooter incidents found that in almost half (21 of 45) the situations in which police engaged the shooter to end the threat, law enforcement suffered casualties, totaling nine killed and 28 wounded (Blair & Schweit 2014). One would assume the danger to an untrained permit holder trying to confront an active shooter would be greater than that of a trained professional, which may in part explain why effective intervention in such cases by permit holders to thwart crime is so rare. Although the same FBI report found that in 21 of a total of 160 active shooter incidents between 2000 and 2013, "the situation ended after unarmed citizens safely and successfully restrained the shooter," there was only one case—in a bar in Winnemucca, Nevada in 2008—in which a private armed citizen other than an armed security guard stopped a shooter, and that individual was an active-duty Marine (Holzel 2008).

ii. The pathway from those harboring criminal intent

Over the 10-year period from May 2007 through January 2017, the Violence Policy Center (2017) lists 31 instances in which concealed carry permit holders killed three or more individuals in a single incident. Many of these episodes are disturbingly similar in that there was substantial evidence of violent tendencies and/or serious mental illness, but no effort was made to even revoke the carry permit, let alone take effective action to prevent access to guns. For example, on January 6, 2017, concealed handgun permit holder Esteban Santiago, 26, killed five and wounded six others at the Fort Lauderdale-Hollywood Airport, before sitting on the floor and waiting to be arrested as soon as he ran out of ammunition. In the year prior to the shooting, police in Anchorage, Alaska, charged Santiago with domestic violence, and visited the home five times for various other complaints (KTUU 2017). In November 2016, Santiago entered the Anchorage FBI office and spoke of "mind control" by the CIA and having "terroristic thoughts" (Hopkins 2017). Although the police took his handgun at the time, it was returned to him on December 7, 2016 after Santiago spent four days in a mental health facility because, according to federal officials, "there was no mechanism in federal law for officers to permanently seize the weapon"[17] (Boots 2017). Less than a month later, Santiago flew with his gun to Florida and opened fire in the baggage claim area.[18]

In January 2018, the FBI charged Taylor Wilson, a 26-year-old Missouri concealed carry permit holder, with terrorism on an Amtrak train when, while carrying a loaded

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[16]In 2012, "a customer with a concealed handgun license … accidentally shot and killed a store clerk" during an attempted robbery in Houston (MacDonald 2012). Similarly, in 2015, also in Houston, a bystander who drew his weapon upon seeing a carjacking incident ended up shooting the victim in the head by accident (KHOU 2015). An episode in June 2017 underscored that interventions even by well-trained individuals can complicate and exacerbate unfolding crime situations. An off-duty Saint Louis police officer with 11 years of service was inside his home when he heard the police exchanging gunfire with some car thieves. Taking his police-issued weapon, he went outside to help, but as he approached he was told by two officers to get on the ground and then shot in the arm by a third officer who "feared for his safety" (Hauser 2017).

[17]Moreover, in 2012, Puerto Rican police confiscated Santiago's handguns and held them for two years before returning them to him in May 2014, after which he moved to Alaska (Clary et al. 2017).

[18]For a similar story of repeated gun violence and signs of mental illness by a concealed carry permit holder, see the case of Aaron Alexis, who murdered 12 at the Washington Navy Yard in September 2013 (Carter et al. 2013).

weapon, he tried to interfere with the brakes and controls of the moving train. According to the FBI, Wilson had (1) previously joined an "alt-right" neo-Nazi group and traveled to the Unite the Right rally in Charlottesville, Virginia in August 2017; (2) indicated his interest in "killing black people" and was the perpetrator of a road-rage incident in which he pointed a gun at a black woman for no apparent reason while driving on an interstate highway in April 2016; and (3) possessed devices and weapons "to engage in criminal offenses against the United States." Research is needed to analyze whether having a permit to legally carry weapons facilitates such criminal designs (Pilger 2018).

In June 2017, Milwaukee Police Chief Ed Flynn pointed out that criminal gangs have taken advantage of RTC laws by having gang members with clean criminal records obtain concealed carry permits and then hold the guns after they are used by the active criminals (Officer.com 2017). Flynn was referring to so-called human holsters who have RTC permits and hold guns for those barred from possession. For example, Wisconsin permit holder Darrail Smith was stopped three times while carrying guns away from crime scenes before police finally charged him with criminal conspiracy. In the second of these, Smith was "carrying three loaded guns, including one that had been reported stolen," but that was an insufficient basis to charge him with a crime or revoke his RTC permit (DePrang 2015). Having a "designated permit holder" along to take possession of the guns when confronted by police may be an attractive benefit for criminal elements acting in concert (Fernandez et al. 2015; Luthern 2015).

b. Increased gun thefts: The most frequent occurrence each year involving crime and a good guy with a gun is not self-defense but rather the theft of the good guy's gun, which occurs hundreds of thousands of times each year.[19] Data from a nationally representative web-based survey conducted in April 2015 of 3,949 subjects revealed that those who carried guns outside the home had their guns stolen at a rate over 1 percent per year (Hemenway et al. 2017). Given the current level of roughly 16 million permit holders, a plausible estimate is that RTC laws result in permit holders furnishing more than 100,000 guns per year to criminals.[20] As Phil Cook has noted, the relationship between gun theft and crime is a complicated one for which few definitive data are currently available (Cook

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[19]According to Larry Keane, senior vice president of the National Shooting Sports Foundation (a trade group that represents firearms manufacturers): "There are more guns stolen every year than there are violent crimes committed with firearms." More than 237,000 guns were reported stolen in the United States in 2016, according to the FBI's National Crime Information Center. The actual number of thefts is obviously much higher since many gun thefts are never reported to police, and "many gun owners who report thefts do not know the serial numbers on their firearms, data required to input weapons into the NCIC." The best survey estimated 380,000 guns were stolen annually in recent years, but given the upward trend in reports to police, that figure likely understates the current level of gun thefts (Freskos 2017b). According to National Crime Information Center data, the number of guns reported stolen nationally jumped 60 percent between 2007 and 2016 (Freskos 2018a).

[20]While the Hemenway et al. study is not large enough and detailed enough to provide precise estimates, it establishes that those who have carried guns in the last month are more likely to have them stolen. A recent Pew Research Survey found that 26 percent of American gun owners say they carry a gun outside of their home "all or most of the time" (Igielnik & Brown 2017, surveying 3,930 U.S. adults, including 1,269 gun owners). If 1 percent of 16 million permit holders have guns stolen each year, that would suggest 160,000 guns were stolen. Only guns stolen outside the home would be attributable to RTC laws, so a plausible estimate of guns stolen per year owing to gun carrying outside the home might be 100,000.

2018). But if there was any merit to the outrage over the loss of about 1,400 guns during the Fast and Furious program that began in 2009 and the contribution that these guns made to crime (primarily in Mexico), it highlights the severity of the vastly greater burdens of guns lost by and stolen from U.S. gun carriers.[21] A 2013 report from the Bureau of Alcohol, Tobacco, Firearms, and Explosives concluded that "lost and stolen guns pose a substantial threat to public safety and to law enforcement. Those that steal firearms commit violent crimes with stolen guns, transfer stolen firearms to others who commit crimes, and create an unregulated secondary market for firearms, including a market for those who are prohibited by law from possessing a gun" (Office of the Director—Strategic Management 2013; Parsons & Vargas 2017).

For example, after Sean Penn obtained a permit to carry a gun, his car was stolen with two guns in the trunk. The car was soon recovered, but the guns were gone (Donohue 2003). In July 2015 in San Francisco, the theft of a gun from a car in San Francisco led to a killing of a tourist on a city pier that almost certainly would not have occurred if the lawful gun owner had not left it in the car (Ho 2015). Just a few months later, a gun stolen from an unlocked car was used in two separate killings in San Francisco and Marin in October 2015 (Ho & Williams 2015). According to the National Crime Victimization Survey, in 2013 there were over 660,000 auto thefts from households. More guns being carried in vehicles by permit holders means more criminals will be walking around with the guns stolen from permit holders.[22]

As Michael Rallings, the top law enforcement official in Memphis, Tennessee, noted in commenting on the problem of guns being stolen from cars: "Laws have unintended consequences. We cannot ignore that as a legislature passes laws that make guns more accessible to criminals, that has a direct effect on our violent crime rate" (Freskos 2017a). An Atlanta police sergeant elaborated on this phenomenon: "Most of our criminals, they go out each and every night hunting for guns, and the easiest way to get them is out of people's cars. We're finding that a majority of stolen guns that are getting in the hands of criminals and being used to commit crimes were stolen out of vehicles" (Freskos 2017c). In 2015, 70 percent of guns reported stolen in Atlanta came from cars and trucks (Freskos 2016). Another Atlanta police officer stated that weapons stolen from cars "are used in crimes to shoot people, to rob people" because criminals find these guns to be easy to steal and hard to trace. "For them, it doesn't cost them anything to break into a car and steal a gun" (Freskos 2016).[23]

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[21]"Of the 2,020 guns involved in the Bureau of Alcohol, Tobacco, Firearms, and Explosives probe dubbed 'Operation Fast and Furious,' 363 have been recovered in the United States and 227 have been recovered in Mexico. That leaves 1,430 guns unaccounted for" (Schwarzschild & Griffin 2011). Wayne LaPierre of the NRA was quoted as saying: "These guns are now, as a result of what [ATF] did, in the hands of evil people, and evil people are committing murders and crimes with these guns against innocent citizens" (Horwitz 2011).

[22]In early December 2017, the sheriff in Jacksonville, Florida announced that his office knew of 521 guns that had been stolen so far in 2017—from unlocked cars alone! (Campbell 2017).

[23]Examples abound: Tario Graham was shot and killed during a domestic dispute in February 2012 with a revolver stolen weeks earlier out of pickup truck six miles away in East Memphis (Perrusquia 2017). In Florida, a handgun stolen from an unlocked Honda Accord in mid-2014 helped kill a police officer a few days before Christmas that year (Sampson 2014). A gun stolen from a parked car during a Mardi Gras parade in 2017 was used a few days later to kill 15-year-old Nia Savage in Mobile, Alabama, on Valentine's Day (Freskos 2017a).

Of course, the permit holders whose guns are stolen are not the killers, but they can be the but-for cause of the killings. Lost, forgotten, and misplaced guns are another dangerous byproduct of RTC laws.[24]

c. Enhancing a culture of violence: The South has long had a higher rate of violent crime than the rest of the country. For example, in 2012, while the South had about one-quarter of the U.S. population, it had almost 41 percent of the violent crime reported to police (Fuchs 2013). Social psychologists have argued that part of the reason the South has a higher violent crime rate is that it has perpetuated a "subculture of violence" predicated on an aggrandized sense of one's rights and honor that responds negatively to perceived insults. A famous experiment published in the *Journal of Personality and Social Psychology* found that southern males were more likely than northern males to respond aggressively to being bumped and insulted. This was confirmed by measurement of their stress hormones and their frequency of engaging in aggressive or dominant behavior after being insulted (Cohen et al. 1996). To the extent that RTC laws reflect and encourage this cultural response, they can promote violent crime not only by permit holders, but by all those with or without guns who are influenced by this crime-inducing worldview.

Even upstanding citizens, such as Donald Brown, a 56-year-old retired Hartford firefighter with a distinguished record of service, can fall prey to the notion that resort to a lawful concealed weapon is a good response to a heated argument. Brown was sentenced to seven years in prison in January 2018 by a Connecticut judge who cited his "poor judgment on April 24, 2015, when he drew his licensed 9mm handgun and fired a round into the abdomen of Lascelles Reid, 33." The shooting was prompted by a dispute "over renovations Reid was performing at a house Brown owns" (Owens 2018). Once again, we see that the RTC permit was the pathway to serious violent crime by a previously law-abiding citizen.

d. Increasing violence by criminals: The argument for RTC laws is often predicated on the supposition that they will encourage good guys to have guns, leading only to benign effects on the behavior of bad guys. This is highly unlikely to be true.[25] Indeed, the

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[24]The growing TSA seizures in carry-on luggage are explained by the increase in the number of gun carriers who simply forget they have a gun in their luggage or briefcase (Williams & Waltrip 2004). A chemistry teacher at Marjory Stoneman Douglas High School in Parkland, Florida, who had said he would be willing to carry a weapon to protect students at the school, was criminally charged for leaving a loaded pistol in a public restroom. The teacher's 9mm Glock was discharged by an intoxicated homeless man who found it in the restroom (Stanglin 2018).

[25]Consider in this regard, David Friedman's theoretical analysis of how right-to-carry laws will reduce violent crime: "Suppose one little old lady in ten carries a gun. Suppose that one in ten of those, if attacked by a mugger, will succeed in killing the mugger instead of being killed by him—or shooting herself in the foot. On average, the mugger is much more likely to win the encounter than the little old lady. But—also on average—every hundred muggings produce one dead mugger. At those odds, mugging is a very unattractive profession—not many little old ladies carry enough money in their purses to justify one chance in a hundred of being killed getting it. The number of muggers—and muggings—declines drastically, not because all of the muggers have been killed but because they have, rationally, sought safer professions" (Friedman 1990). There is certainly no empirical support for the conjecture that muggings will "decline drastically" in the wake of RTC adoption. What Friedman's analysis overlooks is that muggers can decide not to mug (which is what Friedman posits) or they can decide to initiate their muggings by cracking the old ladies over the head or by being

evidence that gun prevalence in a state is associated with higher rates of lethal force by police (even controlling for homicide rates) suggests that police may be more fearful and shoot quicker when they are more likely to interact with an armed individual (Nagin forthcoming).[26] Presumably, criminals would respond in a similar fashion, leading them to arm themselves more frequently, attack more harshly, and shoot more quickly when citizens are more likely to be armed. In one study, two-thirds of prisoners incarcerated for gun offenses "reported that the chance of running into an armed victim was very or somewhat important in their own choice to use a gun" (Cook et al. 2009). Such responses by criminals will elevate the toll of the crimes that do occur.

Indeed, a panel data estimate over the years 1980 to 2016 reveals that the percentage of robberies committed with a firearm rises by 18 percent in the wake of RTC adoption (t = 2.60).[27] Our synthetic controls assessment similarly shows that the percentage of robberies committed with a firearm increases by 35 percent over 10 years (t = 4.48).[28] Moreover, there is no evidence that RTC laws are reducing the overall level of robberies: the panel data analysis associates RTC laws with a 9 percent higher level of overall robberies (t = 1.85) and the synthetic controls analysis suggests a 7 percent growth over 10 years (t = 1.19).

e. Impairing police effectiveness: According to an April 2016 report of the Council of Economic Advisers: "Expanding resources for police has consistently been shown to reduce crime; estimates from economic research suggests that a 10% increase in police size decreases crime by 3 to 10%" (CEA 2016:4). In summarizing the evidence on fighting crime in the *Journal of Economic Literature*, Aaron Chalfin and Justin McCrary note that adding police manpower is almost twice as effective in reducing violent crime as it is in reducing property crime (Chalfin & McCrary 2017). Therefore, anything that RTC laws do to occupy police time, from processing permit applications to checking for permit validity to dealing with gunshot victims, inadvertent gun discharges, and the staggering number of stolen guns is likely to have an opportunity cost expressed in higher violent crime.

The presence of more guns on the street can complicate the job of police as they confront (or shy away from) armed citizens. Daniel Nagin finds a pronounced positive association between statewide prevalence of gun ownership and police use of lethal force (Nagin forthcoming). A Minnesota police officer who stopped Philando Castile for a broken taillight shot him seven times only seconds after Castile indicated he had a permit to carry a weapon because the officer feared the permit holder might be reaching for the

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prepared to shoot them if they start reaching for a gun (or even wear body armor). Depending on the response of the criminals to increased gun carrying by potential victims, the increased risk to the criminals may be small compared to the increased risk to the victims. Only an empirical evaluation can answer this question.

[26]See footnotes 29–31 and accompanying text for examples of this pattern of police use of lethal force.

[27]The panel data model uses the DAW explanatory variables set forth in Table 2.

[28]The weighted average proportion of robberies committed by firearm in the year prior to RTC adoption (for states that adopted RTC between 1981 and 2014) is 36 percent while the similar proportion in 2014 for the same RTC states is 43 percent (and for non-RTC states is 29 percent).

gun. Another RTC permit holder, stranded in his disabled car early one morning on a Florida highway exit ramp, grabbed the gun he had legally purchased three days earlier when a police officer in plainclothes pulled up in a van with tinted windows and no lights. "It was not immediately clear what happened after [the officer] got out of his van, but the permit holder at some point started running … and [the officer] fired six times," killing the permit holder, whose body fell "about 80 to 100 feet from his vehicle," with his undischarged handgun on the ground somewhere in between (Robles & Hauser 2015). After a similar encounter between an officer and a permit holder, the officer asked the gun owner: "Do you realize you almost died tonight?" (Kaste 2019).[29]

A policemen trying to give a traffic ticket has more to fear if the driver is armed. When a gun is found in a car in such a situation, a greater amount of time is needed to ascertain the driver's status as a permit holder. A lawful permit holder who happens to have forgotten his permit may end up taking up more police time through arrest and/or other processing.

Moreover, police may be less enthusiastic about investigating certain suspicious activities or engaging in effective crime-fighting actions given the greater risks that widespread gun carrying poses to them, whether from permit holders or the criminals who steal their guns.[30] In a speech at the University of Chicago Law School in October 2015, then-FBI Director James Comey argued that criticism of overly aggressive policing led officers to back away from more involved policing, causing violent crime to rise (Donohue 2017a). If the more serious concern of being shot by an angry gun toter impairs effective policing, the prospect of increased crime following RTC adoption could be far more substantial than the issue that Comey highlighted.[31]

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[29]A permit to carry instructor has posted a YouTube video about "How to inform an officer you are carrying a handgun and live" that is designed to "keep yourself from getting shot unintentionally" by the police. The video, which has over 4.2 million views, has generated comments from non-Americans that it "makes the US look like a war zone" and leads to such unnatural and time-consuming behavior that "an English officer … would look at you like a complete freak" (Soderling 2016).

[30]"Every law enforcement officer working today knows that any routine traffic stop, delivery of a warrant or court order, or response to a domestic disturbance anywhere in the country involving people of any race or age can put them face to face with a weapon. Guns are everywhere, not just in the inner city" (Wilson 2016). In offering an explanation for why the United States massively leads the developed world in police shootings, criminologist David Kennedy stated: "Police officers in the United States in reality need to be conscious of and are trained to be conscious of the fact that literally every single person they come in contact with may be carrying a concealed firearm." For example, police in England and Wales shot and killed 55 people over the 25-year period from 1990–2014, while in just the first 24 days of 2015, the United States (with six times the population) had a higher number of fatal shootings by police (Lopez 2018).

[31]A vivid illustration of how even the erroneous perception that someone accosted by the police is armed can lead to deadly consequences is revealed in the chilling video of five Arizona police officers confronting an unarmed man they incorrectly believed had a gun. During the prolonged encounter, the officers shouted commands at an intoxicated 26-year-old father of two, who begged with his hands in the air not to be shot. The man was killed by five bullets when, following orders to crawl on the floor toward police, he paused to pull up his slipping pants. A warning against the open carry of guns issued by the San Mateo County, California, Sheriff's Office makes the general point that law enforcement officers become hyper-vigilant when encountering an armed individual: "Should the gun carrying person fail to comply with a law enforcement instruction or move in a way that could be construed as threatening, the police are forced to respond in kind for their own protection. It's well and good in hindsight to say the gun carrier was simply 'exercising their rights' but the result could be deadly" (Lunny 2010).

The presence of multiple gun carriers can also complicate police responses to mass shootings and other crimes. When police arrived at an Alabama mall in November 2018, they saw a 21-year-old concealed carry permit holder with gun drawn, and mistakenly killed him, thinking he was the shooter. In fact, the dead man had been assisting and protecting shoppers, and the real shooter escaped (McLaughlin & Holcombe 2018). Another benign intervention that ended in tragedy for the good guy with a gun occurred in July 2018 when police officers arrived as a "good Samaritan" with a concealed carry permit was trying to break up a fight in Portland, Oregon. The police saw the gun held by the permit holder—a Navy veteran, postal worker, and father of three—and in the confusion shot and killed him (Gueverra 2018).

Good guys with guns also can interfere with police anti-crime efforts. For example, police reported that when a number of Walmart customers (fecklessly) pulled out their weapons during a shooting on November 1, 2017, their "presence 'absolutely' slowed the process of determining who, and how many, suspects were involved in the shootings, said Thornton [Colorado] police spokesman Victor Avila" (Simpson 2017).

Similarly, in 2014, a concealed carry permit holder in Illinois fired two shots at a fleeing armed robber at a phone store, thereby interfering with a pursuing police officer. According to the police: "Since the officer did not know where the shots were fired from, he was forced to terminate his foot pursuit and take cover for his own safety" (Glanton & Sadovi 2014).

Indeed, preventive efforts to get guns off the street in high-crime neighborhoods are less feasible when carrying guns is presumptively legal. The passage of RTC laws normalizes the practice of carrying guns in a way that may enable criminals to carry guns more readily without prompting a challenge, while making it harder for the police to know who is and who is not allowed to possess guns in public.

Furthermore, negligent discharges of guns, although common, rarely lead to charges of violent crime but they can take up valuable police time for investigation and in determining whether criminal prosecution or permit withdrawal is warranted. For example, on November 16, 2017, Tennessee churchgoers were reflecting on the recent Texas church massacre in Sutherland Springs when a permit holder mentioned he always carries his gun, bragging that he would be ready to stop any mass shooter. While proudly showing his Ruger handgun, the permit holder inadvertently shot himself in the palm, causing panic in the church as the bullet "ripped through [his wife's] lower left abdomen, out the right side of her abdomen, into her right forearm and out the backside of her forearm. The bullet then struck the wall and ricocheted, landing under the wife's wheelchair." The gun discharge prompted a 911 call, which in the confusion made the police think an active shooting incident was underway. The result was that the local hospital and a number of schools were placed on lockdown for 45 minutes until the police finally ascertained that the shooting was accidental (Eltagouri 2017).[32]

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[32]Negligent discharges by permit holders have occurred in public and private settings from parks, stadiums, movie theaters, restaurants, and government buildings to private households (WFTV 2015; Heath 2015). Thirty-nine-year-old Mike Lee Dickey, who was babysitting an eight-year-old boy, was in the bathroom removing his handgun from his waistband when it discharged. The bullet passed through two doors, before striking the child in his arm while he slept in a nearby bedroom (Associated Press 2015). In April 2018, a 21-year-old pregnant mother of two in

Everything that takes up added police time or complicates the job of law enforcement will serve as a tax on police, rendering them less effective on the margin, and thereby contributing to crime. Indeed, this may in part explain why RTC states tend to increase the size of their police forces (relative to nonadopting states) after RTC laws are passed, as shown in Table 1.[33]

B. A Simple Difference-in-Differences Analysis

We begin by showing how violent crime evolved over our 1977–2014 data period for RTC and non-RTC states.[34] Figure 1 depicts percentage changes in the violent crime rate over our entire data period for three groups of states: those that never adopted RTC laws, those that adopted RTC laws sometime between 1977 and before 2014, and those that adopted RTC laws prior to 1977. It is noteworthy that the 42.3 percent drop in violent crime in the nine states that never adopted RTC laws is almost an order of magnitude greater than the 4.3 percent reduction experienced by states that adopted RTC laws during our period of analysis.[35]

The NRC Report presented a "no-controls" estimate, which is just the coefficient estimate on the variable indicating the date of adoption of a RTC law in a crime rate panel data model with state and year fixed effects. According to the NRC Report: "Estimating the model using data to 2000 shows that states adopting right-to-carry laws saw 12.9 percent increases in violent crime—and 21.2 percent increases in property crime—relative to national crime patterns." Estimating this same model using 14 additional years of data (through 2014) and 11 additional adopting states (listed at the bottom of Appendix Table C1) reveals that the average postpassage increase in violent crime was

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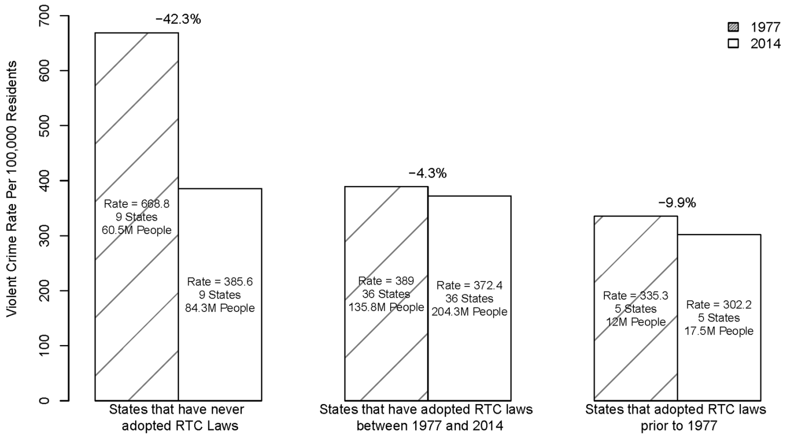
Indiana was shot by her three-year-old daughter when the toddler's father left the legal but loaded 9mm handgun between the console and the front passenger seat after he exited the vehicle to go inside a store. The child climbed over from the backseat and accidentally fired the gun, hitting her mother though the upper right part of her torso. (Palmer 2018) See also Savitsky (2019) (country western singer Justin Carter dies when the gun in his pocket discharges and hits him in the face); Schwarz (2014) (Idaho professor shoots himself in foot during class two months after state legalizes guns on campuses); Murdock (2018) (man shoots himself in the groin with gun in his waistband in the meat section of Walmart in Buckeye, Arizona); Barbash (2018) (California teacher demonstrating gun safety accidentally discharges weapon in a high school classroom in March 2018, injuring one student); Fortin (2018) (in February 2018, a Georgia teacher fired his gun while barricaded in his classroom); US News (2018) (in April 2018, an Ohio woman with a valid concealed carry permit accidentally killed her two-year-old daughter at an Ohio hotel while trying to turn on the gun's safety); and Fox News (2016) ("the owner of an Ohio gun shop was shot and killed when a student in a concealed carry permit class accidentally discharged a weapon," striking the owner in the neck in a different room after the bullet passed through a wall).

[33]See Adda et al. (2014), describing how local depenalization of cannabis enabled the police to reallocate resources, thereby reducing violent crime.

[34]The FBI violent crime category includes murder, rape, robbery, and aggravated assault.

[35]Over the same 1977–2014 period, the states that avoided adopting RTC laws had substantially smaller increases in their rates of incarceration and police employment. The nine never-adopting states increased their incarceration rate by 205 percent, while the incarceration rates in the adopting states rose by 262 and 259 percent, for those adopting RTC laws before and after 1977, respectively. Similarly, the rate of police employment rose by 16 percent in the never-adopting states and by 38 and 55 percent for those adopting before and after 1977, respectively.

Figure 1: The decline in violent crime rates has been far greater in states with no RTC laws, 1977–2014.



Data Sources: UCR for crime rates; Census for state populations.
Note: Illinois excluded since its concealed carry law did not go into effect until 2014. From 1977–2013, the violent crime rate in Illinois fell by 36 percent, from 631 to 403 crimes per 100,000 people.

20.2 percent, while the comparable increase in property crime was 19.2 percent (both having *p* values less than 5 percent).[36]

Of course, it does not prove that RTC laws increase crime simply because RTC states experience a worse postpassage crime pattern. For example, it might be the case that some states decided to fight crime by allowing citizens to carry concealed handguns while others decided to hire more police and incarcerate a greater number of convicted criminals. If police and prisons were more effective in stopping crime, the "no-controls" model might show that the crime experience in RTC states was worse than in other states even if this were not a true causal result of the adoption of RTC laws. As it turns out, though, RTC states not only experienced higher rates of violent crime but they also had larger increases in incarceration and police than other states. Table 1 provides panel data evidence on how incarceration and two measures of police employment changed after RTC adoption (relative to nonadopting states). All three measures rose in RTC states, and the 7–8 percent greater increases in police in RTC states are statistically significant. In other words, Table 1 confirms that RTC states did *not* have relatively declining rates of

[36]The dummy variable model reports the coefficient associated with a RTC variable that is given a value of 0 when a RTC law is not in effect in that year, a value of 1 when a RTC law is in effect that entire year, and a value equal to the portion of the year a RTC law is in effect otherwise. The date of adoption for each RTC state is shown in Appendix Table A1. Note the fact that violent crime was noticeably higher in 1977 in the nine states that did not adopt RTC laws indicates that it will be particularly important that the parallel trends requirement of a valid panel data analysis is established, which is an issue to which we carefully attend in Section III.A.3. All our appendices are posted online at https://works.bepress.com/john_donohue/.

Table 1: Panel Data Estimates Showing Greater Increases in Incarceration and Police Following RTC Adoption: State- and Year-Fixed Effects, and No Other Regressors, 1977–2014

	Incarceration	*Police Employment per 100k*	*Police Officers per 100k*
	(1)	*(2)*	*(3)*
Dummy variable model	6.78 (6.22)	8.39*** (3.15)	7.08** (2.76)

NOTE: OLS estimations include state- and year-fixed effects and are weighted by population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. The police employment and sworn police officer data are from the Uniform Crime Reports (UCR). The source of the incarceration rate is the Bureau of Justice Statistics (2014). *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$. All figures reported in percentage terms.

incarceration or total police employees after adopting their RTC laws that might explain their comparatively poor postpassage crime performance.

III. A Panel Data Analysis of RTC Laws

A. Estimating Two Models on the Full Data Period 1977–2014

We have just seen that RTC law adoption is followed by *higher* rates of violent and property crime (relative to national trends) and that the elevated crime levels after RTC law adoption occur despite the fact that RTC states actually invested relatively more heavily in prisons and police than non-RTC states. While the theoretical predictions about the effect of RTC laws on crime are indeterminate, these two empirical facts based on the actual patterns of crime and crime-fighting measures in RTC and non-RTC states suggest that the most plausible working hypothesis is that RTC laws *increase* crime. The next step in a panel data analysis of RTC laws would be to test this hypothesis by introducing an appropriate set of explanatory variables that plausibly influence crime.

The choice of these variables is important because any variable that both influences crime and is simultaneously correlated with RTC laws must be included if we are to generate unbiased estimates of the impact of RTC laws. At the same time, including irrelevant and/or highly collinear variables can also undermine efforts at valid estimation of the impact of RTC laws. At the very least, it seems advisable to control for the levels of police and incarceration because these have been the two most important criminal justice policy instruments in the battle against crime.

1. The DAW Panel Data Model

In addition to the state and year fixed effects of the no-controls model and the identifier for the presence of an RTC law, our preferred "DAW model" includes an array of other factors that might be expected to influence crime, such as the levels of police and incarceration, various income, poverty, and unemployment measures, and six demographic controls designed to capture the presence of males in three racial categories (black, white, other) in two high-crime age groupings (15–19 and 20–39). Table 2 lists the full

Table 2: Table of Explanatory Variables for Four Panel Data Studies

Explanatory Variables	*DAW*	*LM*
Right-to-carry law	x	x
Lagged per capita incarceration rate	x	
Lagged police staffing per 100,000 residents	x	
Poverty rate	x	
Unemployment rate	x	
Per capita ethanol consumption from beer	x	
Percentage of state population living in metropolitan statistical areas (MSA)	x	
Real per capita personal income	x	x
Real per capita income maintenance		x
Real per capita retirement payments		x
Real per capita unemployment insurance payments		x
Population density		x
Lagged violent or property arrest rate		x
State population		x
6 Age-sex-race demographic variables —all 6 combinations of black, white, and other males in 2 age groups (15–19, 20–39) indicating the percentage of the population in each group	x	
36 Age-sex-race demographic variables —all possible combinations of black, white, and other males in 6 age groups (10–19, 20–29, 30–39, 40–49, 50–64, and over 65) and repeating this all for females, indicating the percentage of the population in each group		x

NOTE: The DAW model is advanced in this article and the LM model was previously published by Lott and Mustard.

set of explanatory variables for both the DAW model and the comparable panel data model used by Lott and Mustard (LM).[37]

Mathematically, the simple dummy model takes the following form:

$$\ln\left(crime\ rate_{it}\right) = \beta X_{it} + \gamma RTC_{it} + \alpha_t + \delta_i + \varepsilon_{it} \tag{1}$$

where γ is the coefficient on the RTC dummy, reflecting the average estimated impact of adopting a RTC law on crime. The matrix X_{it} contains either the DAW or LM covariates

[37]While we attempt to include as many state-year observations in these regressions as possible, District of Columbia incarceration data are missing after the year 2001. In addition, a handful of observations are also dropped from the LM regressions owing to states that did not report any usable arrest data in various years. Our regressions are performed with Huber-White robust standard errors that are clustered at the state level, and we lag the arrest rates used in the LM regression models. The rationales underlying both choices are described in more detail in Aneja et al. (2014). All the regressions presented in this article are weighted by state population.

Table 3: Panel Data Estimates Suggesting that RTC Laws Increase Violent and Property Crime: State- and Year-Fixed Effects, DAW Regressors, 1979–2014

	Murder Rate	*Firearm Murder Rate*	*Nonfirearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Dummy variable model	2.27 (5.05)	2.90 (6.74)	1.53 (3.32)	9.02*** (2.90)	6.49** (2.74)

NOTE: All models include year- and state-fixed effects, and OLS estimates are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. The violent and property crime data are from the Uniform Crime Reports (UCR) while the murder data are from the National Vital Statistics System (NVSS). Six demographic variables (based on different age-sex-race categories) are included as controls in the regression above. Other controls include the lagged incarceration rate, the lagged police employee rate, real per capita personal income, the unemployment rate, poverty rate, beer, and percentage of the population living in MSAs. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$. All figures reported in percentage terms.

and demographic controls for state i in year t. The vectors α and δ are year and state fixed effects, respectively, while ε_{it} is the error term.

The DAW panel data estimates of the impact of RTC laws on crime are shown in Table 3.[38] The results are consistent with, although smaller in magnitude than, those observed in the no-controls model: RTC laws on average increased violent crime by 9.0 percent and property crime by 6.5 percent in the years following adoption.[39] The effect of RTC laws on murder is seen in Table 3 to be very imprecisely estimated and not statistically significant.[40]

We should also note one caveat to our results. Panel data analysis assumes that the treatment in any one state does not influence crime in nontreatment states. However, as we noted above,[41] RTC laws tend to lead to substantial increases in gun thefts and those guns tend to migrate to states with more restrictive gun laws, where they elevate violent crime. This flow of guns from RTC to non-RTC states has been documented by gun trace data (Knight 2013), and Olson et al. (2019) find that "firearm trafficking from states with less restrictive firearm legislation to neighboring states with more restrictive firearm legislation

[38]The complete set of estimates for all explanatory variables (except the demographic variables) for the DAW and LM dummy models are shown in Appendix Table B1.

[39]Defensive uses of guns are more likely for violent crimes because the victim will clearly be present. For property crimes, the victim is typically absent, thus providing less opportunity to defend with a gun. It is unclear whether the many ways in which RTC laws could lead to more crime, which we discuss in Section II.A.2, would be more likely to facilitate violent or property crime, but our intuition is that violent crime would be more strongly influenced, which is in fact what Table 3 suggests.

[40]We thank Phil Cook for informing us that UCR murder data are both less complete and less discerning than murder data collected by the National Vital Statistics. Note that we subtract all cases of justifiable homicides from the murder counts in our own Vital Statistics data.

[41]See text at footnotes 20–22.

increases firearm homicide rates in those restrictive states."[42] As a result, our panel data estimates of the impact of RTC laws are downward biased by the amount that RTC laws induce crime spillovers into non-RTC states.[43] One police investigation revealed that of the 224 guns a single gun trafficker in the DC area was known to have sold in just five months of 2015, 94 were later found at crime scenes from Virginia to New York (Hermann & Weiner 2019).

2. The LM Panel Data Model

Table 2's recitation of the explanatory variables contained in the Lott and Mustard (LM) panel data model reveals there are no controls for the levels of police and incarceration in each state, even though a substantial literature has found that these factors have a large impact on crime. Indeed, as we saw in Table 1, both factors grew substantially and statistically significantly after RTC law adoption. A Bayesian analysis of the impact of RTC laws found that "the incarceration rate is a powerful predictor of future crime rates," and specifically faulted this omission from the Lott and Mustard model (Strnad 2007:201, n.8). We have discussed an array of infirmities with the LM model in Aneja et al. (2014), including their reliance on flawed pseudo-arrest rates, and highly collinear demographic variables.

As noted in Aneja et al. (2014):

> The Lott and Mustard arrest rates … are a ratio of arrests to crimes, which means that when one person kills many, for example, the arrest rate falls, but when many people kill one person, the arrest rate rises, since only one can be arrested in the first instance and many can in the second. The bottom line is that this "arrest rate" is not a probability and is frequently greater than one because of the multiple arrests per crime. For an extended discussion on the abundant problems with this pseudo arrest rate, see Donohue and Wolfers (2009).

The LM arrest rates are also econometrically problematic since the denominator of the arrest rate is the numerator of the dependent variable crime rate, improperly leaving the dependent variable on both sides of the regression equation. We lag the arrest rates by one year to reduce this problem of ratio bias.

Lott and Mustard's use of 36 demographic variables is also a potential concern. With so many enormously collinear variables, the high likelihood of introducing noise into the estimation process is revealed by the wild fluctuations in the coefficient estimates on these variables. For example, consider the LM explanatory variables "neither black nor white male aged 30–39" and the identical corresponding female category. The LM dummy variable model for violent crime suggests that the male group will significantly

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[42]"Seventy-five percent of traceable guns recovered by authorities in New Jersey [a non-RTC state] are purchased in states with weaker gun laws, according to … firearms trace data … compiled by the federal Bureau of Alcohol, Tobacco, Firearms and Explosives … between 2012 and 2016" (Pugliese 2018). See also Freskos (2018b).

[43]Some of the guns stolen from RTC permit holders may also end up in foreign countries, which will stimulate crime there but not bias our panel data estimates. For example, a recent analysis of guns seized by Brazilian police found that 15 percent came from the United States. Since many of these were assault rifles, they were probably not guns carried by American RTC permit holders (Paraguassu & Brito 2018).

Table 4: Panel Data Estimates of the Impact of RTC Laws: State-and Year-Fixed Effects, Using Actual and Modified LM Regressors, 1977–2014

Panel A: LM Regressors Including 36 Demographic Variables					
	Murder Rate	*Firearm Murder Rate*	*Nonfirearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Dummy variable model	–5.17 (3.33)	–3.91 (4.82)	–5.70** (2.45)	–1.38 (3.16)	–0.34 (1.71)
Panel B: LM Regressors with 6 DAW Demographic Variables					
	Murder Rate	*Firearm Murder Rate*	*Nonfirearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Dummy variable model	3.75 (5.92)	4.34 (7.85)	2.64 (4.02)	10.03** (4.81)	7.59** (3.72)
Panel C: LM Regressors with 6 DAW Demographic Variables and Adding Controls for Incarceration and Police					
	Murder Rate	*Firearm Murder Rate*	*Nonfirearm Murder Rate*	*Violent Crime Rate*	*Property Crime Rate*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
Dummy variable model	4.99 (5.50)	5.96 (7.20)	3.76 (4.29)	10.05** (4.54)	8.10** (3.63)

NOTE: All models include year- and state-fixed effects, and OLS estimates are weighted by state population. Robust standard errors (clustered at the state level) are provided next to point estimates in parentheses. In Panel A, 36 demographic variables (based on different age-sex-race categories) are included as controls in the regressions above. In Panel B, only six demographic variables are included. In Panel C, only six demographic variables are included and controls are added for incarceration and police. For all three panels, other controls include the previous year's violent or property crime arrest rate (depending on the crime category of the dependent variable), state population, population density, real per capita income, real per capita unemployment insurance payments, real per capita income maintenance payments, and real retirement payments per person over 65. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. All figures reported in percentage terms.

increase crime (the coefficient is 219), but their female counterparts have an even greater dampening effect on crime (with a coefficient of –258). Both conflicting estimates (not shown in Appendix Table B1) are statistically significant at the 0.01 level, and they are almost certainly picking up noise rather than revealing true relationships. Bizarre results are common in the LM estimates among these 36 demographic variables.[44]

[44]Aneja et al. (2014) test for the severity of the multicollinearity problem using the 36 LM demographic variables, and the problem is indeed serious. The variance inflation factor (VIF) is shown to be in the range of 6 to 7 for the RTC variable in the LM dummy model when the 36 demographic controls are used. Using the six DAW variables reduces the multicollinearity for the RTC dummy to a tolerable level (with VIFs always below the desirable threshold of 5).

Table 4, Panel A shows the results of the LM panel data model estimated over the period 1977–2014. As seen above, the DAW model generated estimates that RTC laws raised violent and property crime (in the dummy model of Table 3), while the estimated impact on murders was too imprecise to be informative. The LM model generates no statistically significant estimates, except for an apparent decline in non-firearm-related murders. We can almost perfectly restore the DAW Table 3 findings, however, by simply limiting the inclusion of 36 highly collinear demographic variables to the more typical array used in the DAW regressions, as seen in Panel B of Table 4. This modified LM dummy variable model suggests that RTC laws increase violent and property crime, mimicking the DAW dummy variable model estimates, and this same finding persists if we add in controls for police and incarceration, as seen in Panel C of Table 4.

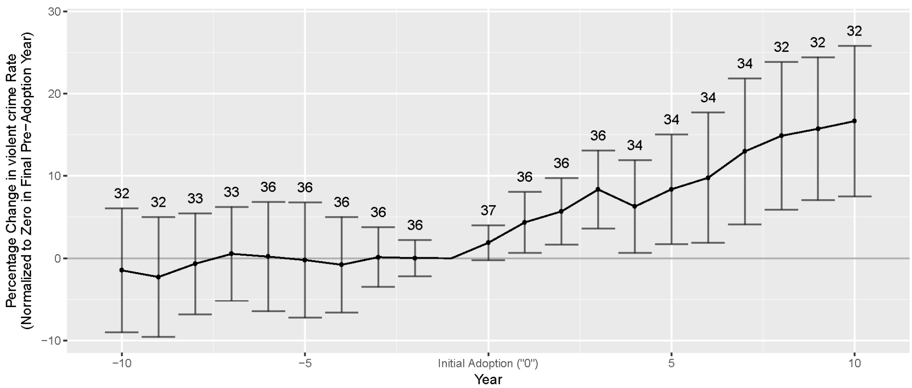
3. Testing the DAW and LM Models for the Parallel Trends Assumption

Many researchers are content to present panel data results such as those shown in Tables 3 and 4 without establishing their econometric validity. This can be a serious mistake. We have already registered concerns about the choice of controls included in the LM model, but, as we will see, the LM model regressions in Panel A of Table 4—including the spurious finding that RTC laws reduce non-firearm homicides—uniformly violate the critical assumption of parallel trends. In sharp contrast, the DAW model illustrates nearly perfect parallel trends in the decade prior to RTC adoption for violent crime and sufficiently satisfies this assumption in three of the other four regressions in Table 3 (murder, non-firearm murder, and property crime).

To implement this test and to provide more nuanced estimates of the impact of RTC laws on crime than in the simple dummy models of Tables 3 and 4, we ran regressions showing the values on yearly dummy variables for 10 years prior to RTC adoption to 10 years after RTC adoption. If the key parallel trends assumption of panel data analysis is valid, we should see values of the pre-adoption dummies that show no trend and are close to zero. Figure 2 shows that the DAW violent crime model performs extremely well: the pre-adoption dummies are virtually all zero (and hence totally flat) for the eight years prior to adoption, and violent crime starts rising in the year of adoption, showing statistically significant increases after the law has been in effect for at least a full year. The upward trend in violent crime continues for the entire decade after adoption. Figure 2 also highlights that the single dummy models of Tables 3 and 4 (which implicitly assume an immediate and constant post-adoption impact on crime) are mis-specified. Importantly, we can now see the exact timing and pattern of the estimated impact on crime, which can, and in this case does, provide further support for a causal interpretation of the estimated increase in violent crime.

In contrast to the ideal performance of the DAW violent crime model, all of the Table 4 regressions using the LM model perform extremely poorly. For example, consider the LM model for firearm murder depicted in Figure 3, which shows that there is

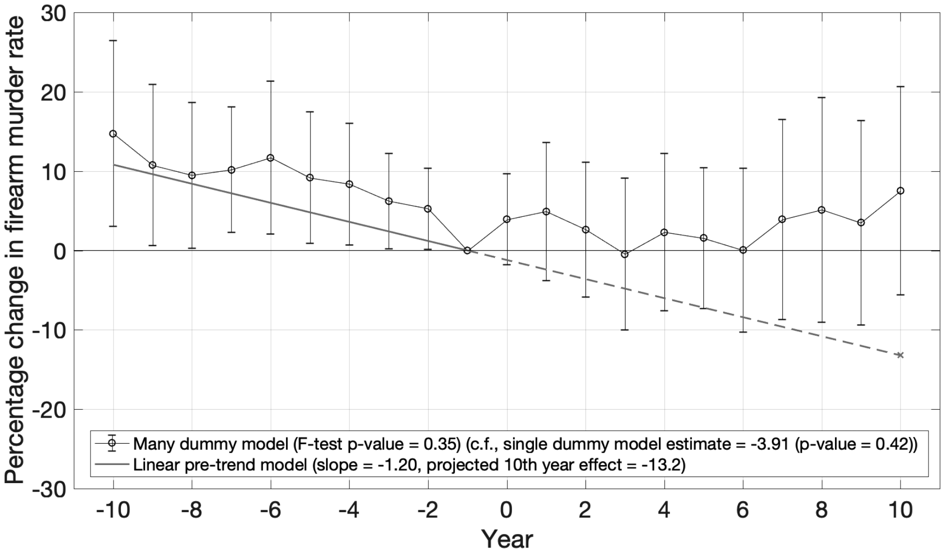
Figure 2: The impact of RTC laws on violent crime, DAW model, 1979–2014.



Note: We regress crime on dummies for pre– and post–passage years and DAW covariates. Reference year is year before adoption and adoption year is first year with RTC in place at any time, meaning that in states that adopt after January 1, this will capture only a partial effect of RTC laws. We display the 95 percent confidence interval for each estimate using cluster-robust standard errors and show the number of states that contribute to each estimate.

an enormously steep downward trend in the values of the pre-adoption dummies. Indeed, we see that the downward trend reverses just at the time of adoption of the RTC law and after six years we observe statistically significant increases in firearm

Figure 3: The impact of RTC laws on firearm murder, LM model, 1977–2014



murder above the prior trend. Thus, while Table 4 ostensibly showed a statistically insignificant 3.9 percent drop in violent crime, the more discerning analysis of Figure 3 shows that that estimate is econometrically invalid, given such an influential violation of the parallel trends requirement. In fact, the LM model estimated for Figure 3 provides evidence that the adoption of RTC laws reversed a previous benign trend starting exactly at the time of RTC adoption and led to higher levels of firearm homicide.

Appendix D depicts the same year-by -year estimates for the other crimes using both the DAW and LM models. It is worth noting that, for our entire data period, the four DAW and LM murder and firearm murder figures show an apparent malign break in trend at the time of RTC adoption, while the trend for non-firearm murder remains unchanged in the DAW and LM models. The unchanged downward trend in the LM non-firearm model illustrates the violation of the parallel trends assumption, invalidating the anomalous finding for that crime in Panel A of Table 4.[45]

For the DAW and LM property crime panel data estimates, we see almost the same pattern. While the pre-adoption performance of the DAW property crime model (see Appendix Figure D2) is not quite as perfect as it was for violent crime, it still shows a roughly flat pattern for the eight years prior to adoption, followed by a persistent pattern of increasing property crime in the 10 years after RTC adoption. The increase in property crime turns statistically significant at the time of adoption. In Appendix Figure D3, however, we again see the same deficient pattern observed for the LM model in Appendix Figure D1: property crime falls in the 10 years prior to adoption, and the pattern reverses itself, leading to increasing property crime in the decade following RTC adoption.

We also conducted a panel data assessment looking at the 11 states that adopted RTC laws in the period from 2000–2014 when the confounding effect of the crack epidemic had subsided. The results provide further support that RTC laws increase crime, including estimates that overall murder and firearm murder rise substantially with RTC adoption. See further discussion and relevant figures and estimates in Appendix C. Figure 4 shows the year-by-year estimated effect of RTC laws on overall murder for the DAW model for this postcrack time period. The figure shows a flat pretrend (albeit with some variance around it) and then a sizeable jump in murder starting just at the year of RTC adoption. The LM model shows substantially the same statistically significant increase in murder.

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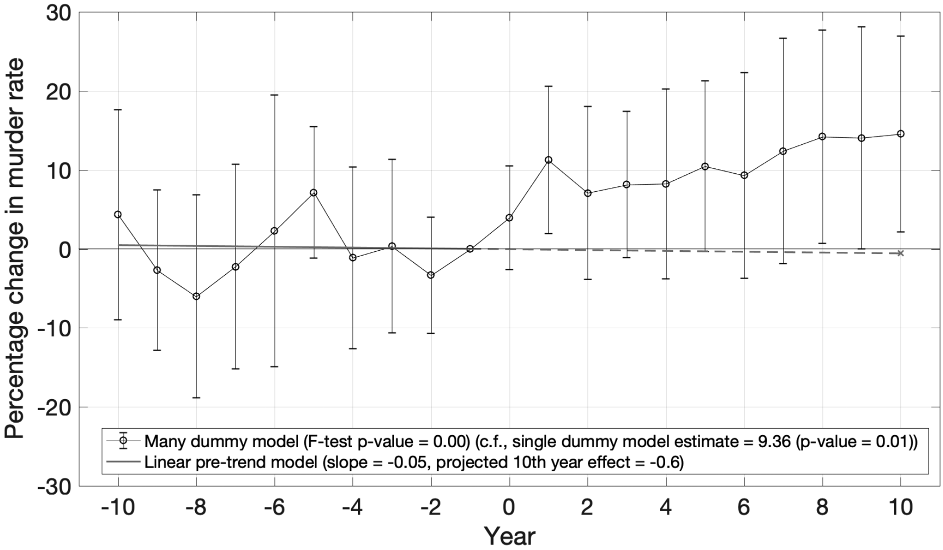
[45]Appendix Figure D1 also illustrates why the LM dummy model estimate on violent crime in Panel A of Table 4 was not positive and statistically significant (as it was for the DAW model in Table 3 and the modified LM models in Panels B and C of Table 4): Appendix Figure D1 reveals that, for the LM model, violent crime was trending down throughout the pre-adoption period, dropping from 5 percentage points to zero over that decade, at which point it reverses and violent crime increases to roughly a 6 percent increase by 10 years after RTC adoption. The v-shape pattern over that two-decade period leads the LM dummy model to obscure the increase in violent crime that is clearly seen in Appendix Figure D1.

Figure 4: The impact of RTC laws on murder, DAW model, 2000–2014



B. *Summary of Panel Data Analysis*

The uncertainty about the impact of RTC laws on crime expressed in the NRC Report was based on an analysis of data only through 2000. The preceding evaluation of an array of different specifications over the full data period from the late 1970s through 2014 as well as in the postcrack period has given consistent evidence that something bad happened to murder and violent and property crime right at the time of RTC adoption. The most statistically significant crime increases for the full period were seen for DAW violent and property crime. For the postcrack period, the largest and most highly statistically significant increases were seen for murder and firearm murder.

Other work has also provided evidence that RTC laws increase murder and/or overall violent crime—see Zimmerman (2014), examining postcrack-era data and the recent work by Donohue (2017b) and Siegel et al. (2017) concluding that RTC laws increase firearm and handgun homicide. Work by McElroy and Wang (2017) reinforces this conclusion, with results from a dynamic model that accounts for forward-looking behavior finding that violent crime would be one-third lower if RTC laws had not been passed. We discuss other recent published studies finding that RTC laws increase violent crime in Appendix C.

Despite the substantial panel data evidence in the post-NRC literature that supports the finding of the pernicious influence of RTC laws on crime, the NRC suggestion that new techniques should be employed to estimate the impact of these laws is fitting. The important paper by Strnad (2007) used a Bayesian approach to argue that none of the published models used in the RTC evaluation literature rated highly in his model selection protocol when applied to data from 1977–1999.

Durlauf et al. attempt to sort out the different specification choices in evaluating RTC laws by using their own Bayesian model averaging approach using county data from 1979–2000. Applying this technique, the authors find that in their preferred spline (trend) model, RTC laws elevate violent crime in the three years after RTC adoption: "As a result of the law being introduced, violent crime increases in the first year and continues to increase afterwards" (2016:50). By the third year, their preferred model suggests a 6.5 percent increase in violent crime. Since their paper only provides estimates for three postpassage years, we cannot draw conclusions beyond this but note that their finding that violent crime increases by over 2 percent per year owing to RTC laws is a substantial crime increase. Moreover, the authors note: "For our estimates, the effect on crime of introducing guns continues to grow over time" (2016:50).[46]

Owing to the substantial challenges of estimating effects from observational data, it will be useful to see if yet another statistical approach that has different attributes from the panel data methodology can enhance our understanding of the impact of RTC laws. The rest of this article will use this synthetic control approach, which has been deemed "arguably the most important innovation in the policy evaluation literature in the last 15 years" (Athey & Imbens 2017).

IV. Estimating the Impact of RTC Laws Using Synthetic Controls

The synthetic control methodology, which is becoming increasingly prominent in economics and other social sciences, is a promising new statistical approach for addressing the impact of RTC laws.[47] While most synthetic control papers focus on a single

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[46]While our analysis focused on crime at the state level, there is obviously heterogeneity in crime rates within states, which is amalgamated into our population-weighted state average figures. A paper by Kovandzic et al. (KMV) buttresses the view that our state-focused estimates are not giving a misleading impression of the impact of RTC laws on violent crime. KMV limited their analysis to urban areas within each state, estimating the impact of RTC laws on crime using a panel data analysis from 1980–2000 on 189 cities with a population of 100,000 or more (Kovandzic et al. 2005). Although they did not estimate an overall violent crime effect, they did report that RTC laws were associated with a highly statistically significant increase in the rate of aggravated assault, the largest single component of violent crime. Their figures suggest that RTC laws led to a 20.1 percent increase in aggravated assault in the 10 years following adoption.

[47]The synthetic control methodology has been deployed in a wide variety of fields, including health economics (Nonnemaker et al. 2011), immigration economics (Bohn et al. 2014), political economy (Keele 2009), urban economics (Ando 2015), the economics of natural resources (Mideksa 2013), and the dynamics of economic growth (Cavallo et al. 2013).

treatment in a single geographic region, we look at 33 RTC adoptions occurring over three decades throughout the country. For each adopting ("treated") state we will find a weighted average of other states ("a synthetic control") designed to serve as a good counterfactual for the impact of RTC laws because it had a pattern of crime similar to that of the adopting state prior to RTC adoption. By comparing what actually happened to crime after RTC adoption to the crime performance of the synthetic control over the same period, we generate estimates of the causal impact of RTC laws on crime.[48]

A. *The Basics of the Synthetic Control Methodology*

The synthetic control method attempts to generate representative counterfactual units by comparing a treatment unit (i.e., a state adopting an RTC law) to a set of control units across a set of explanatory variables over a preintervention period. The algorithm searches for similarities between the treatment state of interest and the control states during this period and then generates a synthetic counterfactual unit for the treatment state that is a weighted combination of the component control states.[49] Two conditions are placed on these weights: they must be nonnegative and they must sum to 1. In general, the matching process underlying the synthetic control technique uses pretreatment values of both the outcome variable of interest (in our case, some measure of crime) and other predictors believed to influence this outcome variable.[50] For the reasons set forth in Appendix K, we use every lag of the dependent variable as predictors in the DAW and LM specifications. Once the synthetic counterfactual is generated and the weights associated with each control unit are assigned, the *synth* program then calculates values for the outcome variable associated with this counterfactual and the root mean squared prediction error (RMSPE) based on differences between the treatment and synthetic control units in the pretreatment period. The effect of the treatment can then be estimated by comparing the actual values of the dependent variable for the treatment unit to the corresponding values of the synthetic control.

B. *Generating Synthetic Controls for 33 States Adopting RTC Laws During Our Data Period*

To illustrate the procedure outlined above, consider the case of Texas, whose RTC law went into effect on January 1, 1996. The potential control group for each treatment state

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[48]For a more detailed technical description of this method, we direct the reader to Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2014).

[49]Our analysis is done in Stata using the *synth* software package developed by Alberto Abadie, Alexis Diamond, and Jens Hainmueller.

[50]Roughly speaking, the algorithm that we use finds $\mathbf{W}$ (the weights of the components of the synthetic control) that minimizes $\sqrt{(\mathbf{X_1} - \mathbf{X_0 W})'\mathbf{V}(\mathbf{X_1} - \mathbf{X_0 W})}$, where $\mathbf{V}$ is a diagonal matrix incorporating information about the relative weights placed on different predictors, $\mathbf{W}$ is a vector of nonnegative weights that sum to 1, $\mathbf{X_1}$ is a vector containing pretreatment information about the predictors associated with the treatment unit, and $\mathbf{X_0}$ is a matrix containing pretreatment information about the predictors for all the control units.

consists of all nine states with no RTC legislation as of the year 2014, as well as states that pass RTC laws at least 10 years after the passage of the treatment state (e.g., in this case, the five states passing RTC laws after 2006, such as Nebraska and Kansas, whose RTC laws went into effect at the beginning of 2007). Since we estimate results for up to 10 years postpassage,[51] this restriction helps us avoid including states with their own permissive concealed carry laws in the synthetically constructed unit (which would mar the control comparison).

After entering the necessary specification information into the *synth* program (e.g., treatment unit, list of control states, explanatory variables, etc.), the algorithm proceeds to construct the synthetic unit from the list of control states specific to Texas and generates values of the dependent variable for the counterfactual for both the pretreatment and posttreatment periods. The rationale behind this methodology is that a close fit in the prepassage time series of crime between the treatment state and the synthetic control generates greater confidence in the accuracy of the constructed counterfactual. Computing the posttreatment difference between the dependent variables of the treatment state and the synthetic control unit provides the synthetic control estimate of the treatment effect attributable to RTC adoption in that state.

1. Synthetic Control Estimates of Violent Crime in Two States

Figure 5 shows the synthetic control graph for violent crime in Texas over the period from 1977 through 2006 (10 years after the adoption of Texas's RTC law). The solid black line shows the actual pattern of violent crime for Texas, and the vertical line indicates when the RTC law went into effect. Implementing the synthetic control protocol identifies three states that generate a good fit for the pattern of crime experienced by Texas in the pre-1996 period. These states are California, which gets a weight of 57.7 percent owing to its similar attributes compared to Texas, Nebraska with a weight of 9.7 percent, and Wisconsin with a weight of 32.6 percent.

One of the advantages of the synthetic control methodology is that one can assess how well the synthetic control (call it "synthetic Texas," which is identified in Figure 5 by the dashed line) matches the pre-RTC-passage pattern of violent crime to see whether the methodology is likely to generate a good fit in the 10 years of postpassage data. Here the fit looks rather good in mimicking the rises and falls in Texas violent crime from 1977–1995. This pattern increases our confidence that synthetic Texas will provide a good prediction of what would have happened in Texas had it not adopted an RTC law.

Looking at Figure 5, we see that while both Texas and synthetic Texas (the weighted average violent crime performance of the three mentioned states) show declining crime rates in the postpassage decade after 1996, the crime drop is

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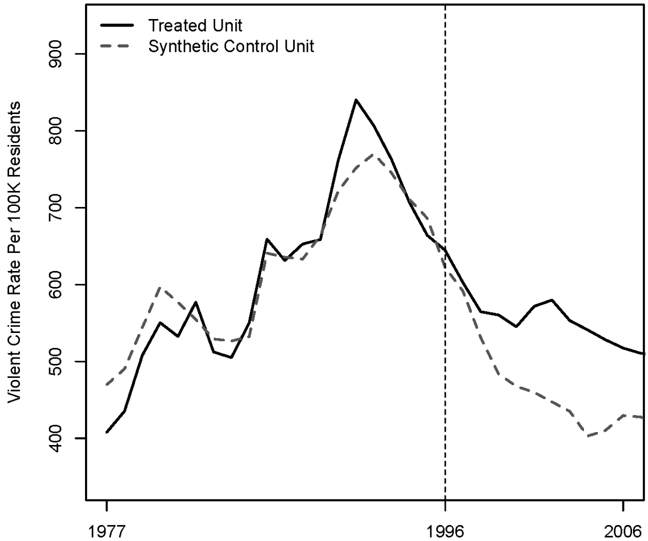
[51]Our choice of 10 years is informed by the tradeoffs associated with using a different timeframe. Tables 5 and 6 indicate that the increase in violent crime due to RTC laws is statistically significant at the .01 level for all years after seven years post-adoption.

Figure 5: Texas: Violent crime rate.



Effect of 1996 RTC Law 10 Years After Adoption: 16.9%
NOTE: Passage Year Difference From SC: 3.6% Composition of SC: CA (0.577); NE (0.097); WI (0.326) CVRMSPE: 0.06 (8 of 33 states, where 1 denotes the state with the best pre-passage fit.).
States Never Passing RTC Laws Included in Synthetic Control: CA;
RTC Adopting States Included in Synthetic Control: NE (2007); WI (2012).

substantially greater in synthetic Texas, which had no RTC law over that period, than in actual Texas, which did. As Figure 5 notes, 10 years after adopting its RTC law, violent crime in Texas was 16.9 percent *higher* than we would have expected had it not adopted an RTC law.[52]

Figure 5 also illustrates perhaps the most important lesson of causal inference: one cannot simply look before and after an event to determine the consequence of the event. Rather, one needs to estimate the difference between what did unfold and the counterfactual of what would have unfolded without the event. The value of the synthetic control methodology is that it provides a highly transparent estimate of that counterfactual, using a tool designed to ensure the validity of the parallel trends assumption that we have already seen is so critical to achieving meaningful causal estimates. Thus, when Lott

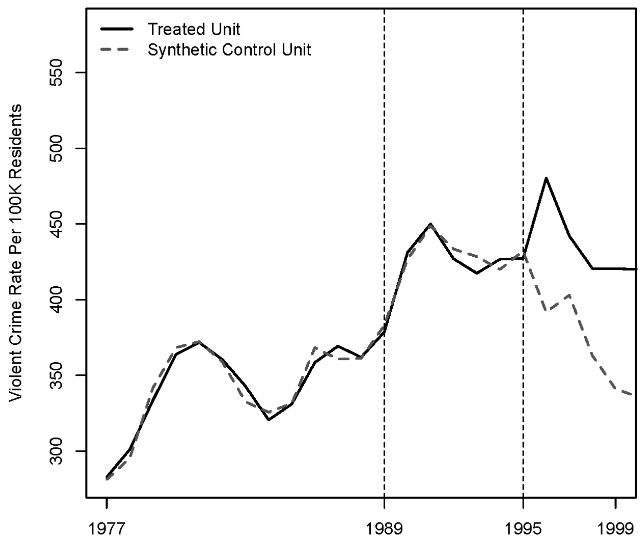
[52]Texas's violent crime rate 10 years post-adoption exceeds that of "synthetic Texas" by 20.41 percent $= \frac{517.3-429.6}{429.6} \times 100\%$. While some researchers would take that value as the estimated effect of RTC, we chose to subtract off the discrepancy in 1996 between the actual violent crime rate and the synthetic control value in that year. This discrepancy is 3.55 percent $= \frac{644.4-622.3}{622.3} \times 100\%$ (shown in the line just below the graph of Figure 5). See footnote 58 for further discussion of this calculation. Figure 5 shows a (rounded) estimated violent crime increase in Texas of 16.9 percent. We arrive at this estimate by subtracting the 1996 discrepancy of 3.55 percent from the 20.41 percent 10th-year discrepancy, which generates a TEP of 16.86 percent.

(2010) quotes a Texas District Attorney suggesting that he had reversed his earlier opposition to the state's RTC law in light of the perceived favorable experience with the law, we see why it can be quite easy to draw the inaccurate causal inference that Texas's crime decline was facilitated by its RTC law. The public may perceive the falling crime rate post-1996 (the solid black line), but our analysis suggests that Texas would have experienced a more sizable violent crime decline if it had not passed an RTC law (the dotted line). More specifically, Texas experienced a 19.7 percent decrease in its aggregate violent crime rate in the 10 years following its RTC law (between 1996 and 2006), while the state's synthetic control experienced a larger 31.0 percent decline. This counterfactual would not be apparent to residents of the state or to law enforcement officials, but our results suggest that Texas's RTC law imposed a large social cost on the state.

The greater transparency of the synthetic control approach is one advantage of this methodology over the panel data models that we considered above. Figure 5 makes clear what Texas is being compared to, and we can reflect on whether this match is plausible and whether anything other than RTC laws changed in these three states during the post-passage decade that might compromise the validity of the synthetic control estimate of the impact of RTC laws.

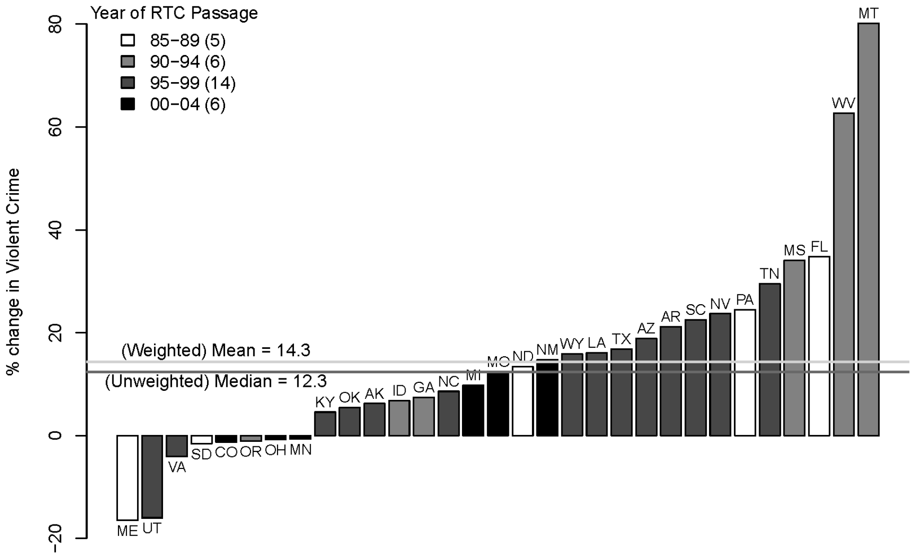
Figure 6 shows our synthetic control estimate for Pennsylvania, which adopted an RTC law in 1989 that did not extend to Philadelphia until a subsequent law went into

Figure 6: Pennsylvania: Violent crime rate.



Effect of 1989 RTC Law 10 Years After Adoption: 24.4%
NOTE: Passage Year Difference From SC: -1.1%. Composition of SC: DE (0.078); HI (0.073); MD (0.038); NE (0.016); NJ (0.103); OH (0.27); WI (0.424) CVRMSPE: 0.017 (1 of 33 states, where 1 denotes the state with the best pre-passage fit.).
States Never Passing RTC Laws Included in Synthetic Control: DE; HI; MD; NJ;
RTC Adopting States Included in Synthetic Control: NE (2007); OH (2004); WI (2012).

Figure 7: The effect of RTC laws on violent crime after 10 years, synthetic control estimates for 31 states (1977–2014).



effect on October 11, 1995. In this case, synthetic Pennsylvania is comprised of eight states and the prepassage fit is nearly perfect. Following adoption of the RTC laws, synthetic Pennsylvania shows substantially better crime performance than actual Pennsylvania after the RTC law is extended to Philadelphia in late 1995, as illustrated by the second vertical line at 1996. The synthetic control method estimates that RTC laws in Pennsylvania increased its violent crime rate by 24.4 percent after 10 years.[53]

2. State-Specific Estimates Across All RTC States

Because we are projecting the violent crime experience of the synthetic control over a 10-year period, there will undoubtedly be a deviation from the "true" counterfactual and our estimated counterfactual. If we were only estimating the impact of a legal change for a single state, we would have an estimate marred by this purely stochastic aspect of changing crime. Since we are estimating an average effect across a large number of states, the

[53]In Appendix I, we include all 33 graphs showing the path of violent crime for the treatment states and the synthetic controls, along with information about the composition of these synthetic controls, the dates of RTC adoption (if any) for states included in these synthetic controls, and the estimated treatment effect (expressed in terms of the percent change in a particular crime rate) 10 years after adoption (or seven years after adoption for two states that adopted RTC laws in 2007, since our data end in 2014). The figures also document the discrepancy in violent crime in the year of adoption between the actual and synthetic control values.

stochastic variation will be diminished as the overestimates and underestimates will tend to wash out in our mean treatment estimates. Figure 7 shows the synthetic control estimates on violent crime for all 31 states for which we have 10 years of postpassage data. For 23 of the 31 states adopting RTC laws, the increase in violent crime is noteworthy.[54] Although three states were estimated to have crime reductions greater than the –1.6 percent estimate of South Dakota, if one averages across all 31 states, the (population-weighted) mean treatment effect after 10 years is a 14.3 percent *increase* in violent crime. If one instead uses an (unweighted) median measure of central tendency, RTC laws are seen to *increase* crime by 12.3 percent.

3. Less Effective Prepassage Matches

Section IV.B.1 provided two examples of synthetic controls that matched the crime of the treatment states well in the prepassage period, but this does not always happen. For example, we would have considerably less confidence in the quality of the synthetic control estimates for Maine, whose poor estimate is depicted in Appendix Figure I11. Maine also happens to be the state showing the greatest reduction in violent crime following RTC adoption, as indicated in Figure 7.

For Maine, one sees that the synthetic control and the state violent crime performance diverged long before RTC adoption in 1986, and that, by the date of adoption, Maine's violent crime rate was already 37.9 percent below the synthetic control estimate. The violent crime rate of actual Maine was trending down, while the synthetic control estimate had been much higher and trending up in the immediate pre-adoption period. The difficulty in generating good prepassage matches for states like Maine stems from their unusually low violent crime in the prepassage period.

Appendix Figure D11 reproduces Figure 7 while leaving out the five states for which the quality of prepassage fit is clearly lower than in the remaining 26 states.[55] This knocks out North Dakota, South Dakota, Maine, Montana, and West Virginia, thereby eliminating three of the five outlier estimates at both ends of the scale, and leaving the mean and median effects of RTC laws relatively unchanged from Figure 7. As Appendix Figure D11 shows, the (weighted) mean increase in crime across the listed 26 RTC-adopting states is 13.7 percent while the (unweighted) median increase is now 11.1 percent. Increases in violent crime of this magnitude are troubling. Consensus estimates of the elasticity of crime with respect to incarceration hover around 0.15 today, which suggests that to offset the increase in crime caused by RTC adoption, the average RTC state would need to approximately double its prison population.

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[54]The smallest of these, Kentucky, had an increase of 4.6 percent.

[55]In particular, for these five states, the prepassage CVRMSPE—that is, the RMSPE transformed into a coefficient of variation by dividing by the average prepassage crime rate—was 19 percent or greater. See note 61 for further discussion of this statistic.

V. Aggregation Analysis Using Synthetic Controls

A small but growing literature applies synthetic control techniques to the analysis of multiple treatments.[56] We estimate the percentage difference in violent crime between each treatment (RTC-adopting) state and the corresponding synthetic control in both the year of the treatment and in the 10 years following it. This estimate of the treatment effect percentage (TEP) obviously uses data from fewer posttreatment years for the two treatment states[57] in which RTC laws took effect less than 10 years before the end of our sample.

We could use each of these 10 percentage differences as our estimated effects of RTC laws on violent crime for the 10 postpassage years, but, as noted above, we make one adjustment to these figures by subtracting from each the percentage difference in violent crime in the adoption year between the treatment and synthetic control states. In other words, if 10 years after adopting an RTC law, the violent crime rate for the state was 440 and the violent crime rate for the synthetic control was 400, one estimate of the effect of the RTC law could be 10 percent $\left(= \frac{440 - 400}{400}\right)$. Rather than use this estimate, however, we have subtracted from this figure the percentage difference between the synthetic and treatment states in the year of RTC adoption. If, say, the violent crime rate in the treatment state that year was 2 percent higher than the synthetic control value, we would subtract 2 from 10 to obtain an estimated 10th-year effect of RTC laws of 8 percent.[58] We

[56]The closest paper to the present study is Arindrajit Dube and Ben Zipperer (2013), who introduce their own methodology for aggregating multiple events into a single estimated treatment effect and calculating its significance. Their study centers on the effect of increases in the minimum wage on employment outcomes, and, as we do, the authors estimate the percentage difference between the treatment and the synthetic control in the posttreatment period. While some papers analyze multiple treatments by aggregating the areas affected by these treatments into a single unit, this approach is not well-equipped to deal with a case such as RTC law adoption where treatments affect the majority of panel units and more than two decades separate the dates of the first and last treatment under consideration, as highlighted in Figure 7.

[57]These two states are Kansas and Nebraska, which adopted RTC laws in 2007. See note 4 discussing the states for which we cannot estimate the impact of RTC laws using synthetic controls.

[58]It is unclear ex ante whether one should implement this subtraction. The intuitive rationale for our choice of outcome variable was that pretreatment differences between the treatment state and its synthetic control at the time of RTC adoption likely reflected imperfections in the process of generating a synthetic control and should not contribute to our estimated treatment effect if possible. In other words, if the treatment state had a crime rate that was 5 percent greater than that of the synthetic control in both the pretreatment and posttreatment period, it would arguably be misleading to ignore the pretreatment difference and declare that the treatment increased crime rates by 5 percent. On the other hand, subtracting off the initial discrepancy might be adding noise to the subsequent estimates.

We resolve this issue with the following test of our synthetic control protocol: we pretend that each RTC-adopting state actually adopted its RTC law five years before it did. We then generate synthetic control estimates of this phantom law over the next five years of actual pretreatment data. If our synthetic control approach is working perfectly, it should simply replicate the violent crime pattern for the five pretreatment years. Consequently, the estimated "effect" of the phantom law should be close to zero. Indeed, when we follow our subtraction protocol, the synthetic controls match the pretreatment years more closely than when we do not provide this normalization. Specifically, with subtraction the estimated "effect" in the final pretreatment year is a wholly insignificant 3.2 percent; without subtraction, it jumps to a statistically significant 5.3 percent. Consequently, then look across all the state-specific estimates of the impact of RTC laws on violent crime for each of the 10 individual postpassage years and test whether they are significantly different from zero.[59]

A. RTC Laws Increase Violent Crime

We begin our analysis of the aggregated synthetic control results using predictors derived from the DAW specification. Table 5 shows our results on the full sample examining violent crime.[60] Our estimates of the normalized average treatment effect percentage (TEP) suggest that states that passed RTC laws experienced more deleterious changes in violent criminal activity than their synthetic controls in the 10 years after adoption. On average, treatment states had aggregate violent crime rates that were almost 7 percent higher than their synthetic controls five years after passage and around 14 percent higher 10 years after passage. Table 5 suggests that the longer the RTC law is in effect (up to the 10th year that we analyze), the greater the cost in terms of increased violent crime.

As we saw in Figures 6 (Pennsylvania) and I11(Maine), the validity of using the posttreatment difference between crime rates in the treatment state (the particular state adopting an RTC law that we are analyzing) and its corresponding synthetic control as a measure of the effect of the RTC law depends on the strength of the match between these two time series in the pretreatment period. To generate an estimate of pretreatment fit that takes into account differences in pretreatment crime levels, we estimate the coefficient of variation for the root mean squared prediction error (RMSPE), which

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normalization is the preferred approach for violent crime. It should also be noted that our actual synthetic control estimates will be expected to perform better than this phantom RTC estimate since we will be able to derive our synthetic controls from five additional years of data, thereby improving our pretreatment fit.

As it turns out, the choice we made to subtract off the initial-year crime discrepancy is a conservative one, in that the estimated crime increases from RTC laws would be *greater* without subtraction. We provide synthetic control estimates for the DAW model without subtraction of the adoption-year percentage difference for violent crime, murder, and property crime in Appendix F. Comparison of these Appendix F estimates with those in the text (Table 5) reveals that our preferred method of subtracting yields more conservative results (i.e., a smaller increase in violent crime due to RTC). In Table 5, we estimate the 10th-year TEP for violent crime as roughly 13.5 to 14.3 percent, while the comparable estimates without subtraction are roughly 17–18 percent, as seen in Appendix Tables F1, F2, and F3. Indeed, without subtraction, every estimated impact would show RTC laws lead to a statistically significant increase in every crime category we consider except non-firearm homicide, as seen in Appendix F.

[59]This test is performed by regressing these differences in a model using only a constant term and examining whether that constant is statistically significant. These regressions are weighted by the population of the treatment state in the posttreatment year under consideration. Robust standard errors corrected for heteroskedasticity are used in this analysis.

[60]We discuss the synthetic control estimates for murder and property crime in Section V.F.

Table 5: The Impact of RTC Laws on the Violent Crime Rate, DAW Covariates, Full Sample, 1977–2014

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
Average normalized treatment	–0.117	2.629*	3.631*	4.682**	6.876***	7.358**	10.068***	12.474***	14.021***	14.344***
effect percentage (TEP)	(1.076)	(1.310)	(1.848)	(2.068)	(2.499)	(3.135)	(2.823)	(3.831)	(3.605)	(2.921)
N	33	33	33	33	33	33	33	31	31	31
Pseudo p value	0.936	0.274	0.220	0.192	0.094	0.106	0.060	0.038	0.032	0.032

NOTE: Standard errors in parentheses. Column numbers indicate postpassage year under consideration; N = number of states in sample. The synthetic controls method is run using the nested option, and each year's estimate and statistical significance is computed as explained in note 59. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

is the ratio of the synthetic control's pretreatment RMSPE to the pretreatment average level of the outcome variable for the treatment state.[61]

To evaluate the sensitivity of the aggregate synthetic control estimate of the crime impact of RTC laws in Table 5, we consider two subsamples of treatment states: states whose coefficients of variation are less than two times the average coefficient of variation for all 33 treatments and states whose coefficients of variation are less than this average. We then rerun our synthetic control protocol using each of these two subsamples to examine whether restricting our estimation of the average treatment effect to states for which a relatively "better" synthetic control could be identified would meaningfully change our findings.

All three samples yield roughly identical conclusions: RTC laws are consistently shown to increase violent crime, with the 10th-year increase ranging from a low of 13.5 (when we remove the six states with above-average values of the CV RMSPE) to a high of 14.3 percent (Table 5).

B. The Placebo Analysis

Our ability to make valid inferences from our synthetic control estimates depends on the accuracy of our standard error estimation. To test the robustness of the standard errors that we present under the first row of Table 5, we incorporate an analysis using placebo treatment effects similar to Ando (2015).[62] For this analysis, we generate 500 sets of randomly generated RTC dates that are designed to resemble the distribution of actual RTC

[61]While the RMSPE is often used to assess this fit, we believe that the use of this measure is not ideal for comparing fit across states, owing to the wide variation that exists in the average pretreatment crime rates among the 33 treatment states that we consider. For example, the pretreatment RMPSE associated with our synthetic control analysis using the DAW predictor variables and aggregate violent crime as the outcome variable is nearly identical for Texas (37.1) and Maine (36.4), but the pretreatment levels of Texas's aggregate violent crime rate are far greater than Maine's. To be more specific, Texas's average violent crime rate prior to the implementation of its RTC law (from 1977 through 1995) was 617 violent crimes per 100,000 residents, while the corresponding figure for Maine was 186 violent crimes per 100,000 residents, less than one-third of Texas's rate. The more discerning CV of the RMSPE is 0.06 for Texas (with a year of adoption discrepancy of only 3.6 percent), while for Maine, the CV is a dramatically higher at 0.196 (with an initial year discrepancy of –37.9 percent). Accordingly, since the percentage imprecision in our synthetic pretreatment match for Maine is so much greater than for Texas, we have greater confidence in our estimates that in the 10th year, Texas's RTC law had increased violent crime by 16.9 percent than we do in an estimate that Maine's law had decreased violent crime by 16.5 percent.

[62]Ando (2015) examines the impact of constructing nuclear plants on local real per capita taxable income in Japan by generating a synthetic control for every coastal municipality that installed a nuclear plant. Although the average treatment effect measured in our article differs from the one used by Ando, we follow Ando in repeatedly estimating average placebo effects by randomly selecting different areas to serve as placebo treatments. (The sheer number of treatments that we are considering in this analysis prevents us from limiting our placebo treatment analysis to states that never adopt RTC laws, but this simply means that our placebo estimates will likely be biased *against* finding a qualitatively significant effect of RTC laws on crime, since some of our placebo treatments will be capturing the effect of the passage of RTC laws on crime rates.) Our estimated average treatment effect can then be compared to the distribution of average placebo treatment effects. Heersink and Peterson (2016) and Cavallo et al. (2013) also perform a similar randomization procedure to estimate the significance of their estimated average treatment effects, although the randomization procedure in the latter paper differs from ours by restricting the timing of placebo treatments to the exact dates when actual treatments took place.

passage dates that we use in our analysis.[63] For each of the 500 sets of randomly generated RTC dates, we then use the synthetic control methodology and the DAW predictors to estimate synthetic controls for each of the 33 states whose randomly generated adoption year is between 1981 and 2010. We use these data to estimate the percentage difference between each placebo treatment and its corresponding synthetic control during both the year of the treatment and each of the 10 posttreatment years (for which we have data) that follow it. Using the methodology described in notes 52 and 58, we then test whether the estimated treatment effect for each of the 10 posttreatment years is statistically significant.

To further assess the statistical significance of our results, we compare each of the 10 coefficient estimates in Table 5 with the distribution of the 500 average placebo treatment effects that use the same crime rate, posttreatment year, and sample as the given estimate. To assist in this comparison process, we report a pseudo *p* value that is equal to the proportion of our placebo treatment effects whose absolute value is greater than the absolute value of the given estimated treatment effect. This pseudo *p* value provides another intuitive measure of whether our estimated average treatment effects are qualitatively large compared to the distribution of placebo effects. Our confidence that the treatment effect that we are measuring for RTC laws is real increases if our estimated treatment effect is greater than the vast majority of our estimated average placebo treatment effects. Examining our pseudo *p* values in Table 5, we see that our violent crime results are always statistically significant in comparison to the distribution of placebo coefficients at the 0.05 level eight years or more past RTC adoption.

C. *Synthetic Control Estimates Using LM's Explanatory Variables*

In our Section III panel data analysis, we saw that RTC laws were associated with significantly higher rates of violent crime in the DAW model (Table 3), but not in the LM model (Table 4, Panel A). Under the synthetic controls approach, however, we find that the results are the same whether one uses the DAW or LM explanatory variables. This is necessarily true when one uses yearly lags in implementing the synthetic controls – see Kaul et al. (2016) – but it is also true when we use three lags of the dependent variable in our synthetic control protocol, as shown in Table 6. The detrimental effects of RTC laws on violent crime rates are statistically significant at the 0.05 level starting three years after the passage of an RTC law, and appear to increase over time. The treatment effects associated with violent crime in Table 6 range from 9.6 percent in the seventh posttreatment year to 12.8 percent in the 10th posttreatment year. Remarkably, the DAW and LM synthetic control estimates of the impact of RTC laws on violent crime are nearly identical

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[63]More specifically, we randomly choose eight states to never pass RTC laws, six states to pass RTC laws before 1981, 33 states to pass RTC laws between 1981 and 2010, and three states to pass their RTC laws between 2011 and 2014. (Washington, DC is not included in the placebo analysis since it is excluded from our main analysis.) These figures were chosen to mirror the number of states in each of these categories in our actual data set.

Table 6: The Impact of RTC Laws on the Violent Crime Rate, LM covariates, Full Sample, 1977–2014

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
Average Normalized TEP	0.309	1.981	4.063*	5.211*	7.159**	6.981**	9.644***	11.160***	12.115***	12.794***
	(1.318)	(1.646)	(2.192)	(2.572)	(2.887)	(3.319)	(3.016)	(3.680)	(3.857)	(3.200)
N	33	33	33	33	33	33	33	31	31	31

NOTE: Standard errors in parentheses. Column numbers indicate post-passage year under consideration; N = number of states in sample. The synthetic controls method is run using the non-nested option, and each year's estimate and statistical significance is computed as explained in footnote 59. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

(compare Tables 6 and Appendix Table K1), and this is true even when we limit the sample of states in the manner described above.[64]

D. The Contributions of Donor States to the Synthetic Control Estimates: Evaluating Robustness

One of the key elements of the synthetic control approach is its selection among plausible control states. For each state adopting an RTC law in year X, the approach selects among states that do not have RTC laws through at least ten years after X, including never-adopting states. Appendix Figure D10 lists all the states that are eligible under this criterion to serve as synthetic controls for one or more of the 33 adopting states, and shows how often they are selected. The horizontal length of each bar tells us how much that state contributes to our synthetic control violent crime estimates.[65] As the figure indicates, Hawaii appears most frequently—contributing to a synthetic control 18 of the 33 times it is eligible and averaging a 15.2 percent contribution—but California, a substantial contributor to multiple large states, edges it out for the largest average contribution (18.1 percent).

Hawaii's relatively large contribution as a donor state in the synthetic control estimates has some advantages but also raises concern that this small state might be unrepresentative of the states for which it is used as a control. For example, note that the largest share of Virginia's synthetic control comes from Hawaii (27.9 percent), with Rhode Island, Kansas, and Nebraska making up the lion's share of the remaining synthetic control. We had already mentioned one problem with the panel data analysis caused by the tendency of lax gun control states to serve as a source for guns that contribute to crime in the non-RTC states, and Virginia has always been a major source of that interstate flow. Since Virginia's guns are not likely to end up in Hawaii, the bias that the treatment infects the control is reduced for that particular match. Nonetheless, one may be concerned that Hawaii might be unduly skewing the estimates of the impact of RTC laws on violent crime.

To address this, as well as the analogous concern for other potentially idiosyncratic control states, we generated 18 additional TEP estimates, with each one generated by dropping a single one of the 18 states that appears as an element of our synthetic control analysis (as identified in Appendix Figure D10). The results of this exercise are presented in Appendix Figure D12, which shows that our estimated increase in violent crime resulting from the adoption of an RTC law is extremely robust: All 18 estimates remain statistically significant at the 0.01 percent level, and

[64]The 10th-year effect in the synthetic control analysis using the LM variables is 12.4 percent when we eliminate the three states with more than twice the average CV of the RMSPE. Knocking out the seven states with above-average values of this CV generates a similar 12.5 percent effect.

[65]In particular, it reflects the portion of each synthetic state it becomes part of, weighted by the treated state's population. For example, Texas's population is 13.6 percent of the total treated states' population. As a result, a state that made up 50 percent of synthetic Texas (but is not a donor for any other treatment state) would have a bar of size 6.8 percent.

the smallest TEP, which comes from dropping Illinois as a control state, is 12.0 percent. Note in particular that dropping Hawaii from the list of potential donor states slightly *increases* the estimate of the increase in violent crime caused by RTC laws. In fact, when we dropped Hawaii completely as a potential control and repeated the previous protocol of dropping one state at a time, the estimated increase in violent crime from RTC never fell below 12 percent (which was the value when New York was dropped as well as Hawaii). Indeed, the synthetic control finding that RTC laws increase violent crime is so robust that even if we drop California, New York, and Hawaii from the pool of potential donor states, RTC laws still increase violent crime by 8.9 percent after 10 years ($p = 0.018$).

E. Does Gun Prevalence Influence the Impact of RTC Laws?

The wide variation in the state-specific synthetic control estimates that was seen in Figures 7 and D11 suggests that there is considerable noise in some of the outlier estimates of a few individual states. For example, it is highly improbable that RTC laws led to a 16.5 percent decrease in violent crime in Maine and an 80.2 percent increase in violent crime in Montana, the two most extreme estimates seen in Figure 7. Since averaging across a substantial number of states will tend to eliminate the noise in the estimates, one should repose much greater confidence in the aggregated estimates than in any individual state estimate. Indeed, the fact that we can average across 33 separate RTC-adopting states is what generates such convincing and robust estimates of the impact of RTC laws on violent crime.

Another way to distill the signal from the noise in the state-specific estimates is to consider whether there is a plausible factor that could explain underlying differences in how RTC adoption influences violent crime. For example, RTC laws might influence crime differently depending on the level of gun prevalence in the state.

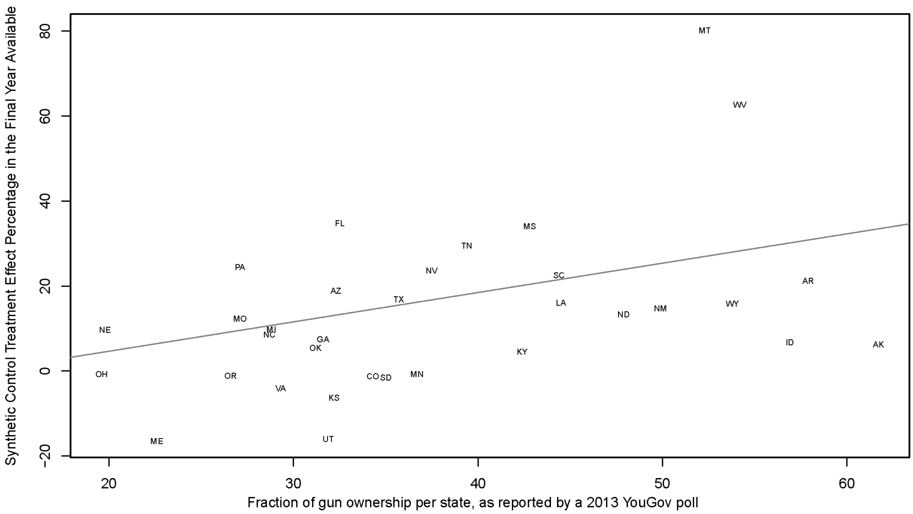
Figure 8 shows the scatter diagram for 33 RTC-adopting states, and relates the estimated impact on violent crime to a measure of gun prevalence in each RTC-adopting state. The last line of the note below the figure provides the regression equation, which shows that gun prevalence is positively related to the estimated increase in crime ($t = 2.39$).[66]

F. The Murder and Property Crime Assessments with Synthetic Controls

The synthetic control estimates of the impact of RTC laws on violent crime uniformly generate statistically significant estimates, and our phantom RTC law synthetic control estimates for the five pretreatment years (described in note 58) give us confidence that the synthetic control approach is working well for our violent crime estimates, as illustrated in Appendix Table L1. Since the estimated increases in violent crime are

[66]The gun prevalence data were collected by the data analytics firm YouGov in a 2013 online survey (Kalesan et al. 2016); 4,486 people were initially surveyed, although only 4,000 results are used in the final data set. YouGov used a proximity matching method to select the survey results for inclusion, matching respondents by race, age, gender, and education to the demographic breakdown of the 2010 American Community Survey.

Figure 8: The impact of gun ownership on the increase in violent crime due to RTC laws (synthetic control estimates, 1977–2014).



NOTE: Treatment effect displayed is for the 10th year after RTC adoption (but 7th post–passage year for Kansas and Nebraska). Treatment Effect = –9.15 + 0.69 * Gun Prevalence. t = 2.39; R^2 = 0.16. Regression weighted by population in the final TEP year.

statistically significant and consistently observed in both our panel data and synthetic control analyses, these represent our most robust finding.

Just as we saw in the panel data analysis, the synthetic controls provide evidence of increases in the murder and firearm murder categories, but it is weaker and less precise than our violent crime estimates. For example, both Appendix Tables E1 and E2 show estimated crime increases of 8.7 percent (murder) and 15.3 percent (firearm murder), but only the 8.7 figure is statistically significant at the 0.10 level. Interestingly, our phantom law test works well for murder and even suggests statistically significant increases in that crime beginning right at the time of RTC adoption (Appendix Table L3). The firearm murder estimates perform less well in this test, generating an estimated fall in crime of 6.8 percent in the year prior to RTC adoption (Appendix Table L5).

The results from implementing this phantom law approach for property crime are perhaps our less encouraging estimates. While our estimated "effect" in the year prior to adoption would ideally be close to zero in this test, for property crime it is 6.9 percent, with the latter significant at the 0.10 level. (The full results of this test for all the crime categories are shown in Appendix L.) If we accept our normalized estimate for the impact of RTC laws on property crime it would give little reason to reject a null hypothesis of no effect (Appendix Table E8). Because our synthetic control estimates for violent crime are validated by our phantom adoption test and generate uniform and highly robust results whether dropping selected donor states or states with poor fit, or using either the DAW or LM models, we have greater confidence in and therefore highlight our violent crime estimates. Accordingly, we consign our further discussion of the synthetic control estimates of murder and property crime to Appendix E.

VI. Conclusion

The extensive array of panel data and synthetic control estimates of the impact of RTC laws that we present uniformly undermine the "More Guns, Less Crime" hypothesis. There is not even the slightest hint in the data from any econometrically sound regression that RTC laws reduce violent crime. Indeed, the weight of the evidence from the panel data estimates as well as the synthetic control analysis best supports the view that the adoption of RTC laws substantially raises overall violent crime in the 10 years after adoption.

In our initial panel data analysis, our preferred DAW specification predicted that RTC laws have led to statistically significant and substantial increases in violent crime. We also presented both panel data and synthetic control estimates that RTC laws substantially increase the percentage of robberies committed with a firearm, while having no restraining effect on the overall number of robberies. Moreover, to the extent the massive theft of guns from carrying guns outside the home generates crime spillovers to non-RTC states, our estimated increases in violent crime are downward biased.

We then supplemented our panel data results using our synthetic control methodology, and the finding from our panel data analysis was strongly buttressed. Whether we used the DAW or LM specifications, states that passed RTC laws experienced 13–15 percent *higher* aggregate violent crime rates than their synthetic controls after 10 years (results that were significant at either the 0.05 or 0.01 level after five years).

The synthetic control effects that we measure represent meaningful increases in violent crime rates following the adoption of RTC laws, and this conclusion remained unchanged after restricting the set of states considered based on model fit and after considering a large number of robustness checks. The consistency across different specifications and methodologies of the finding that RTC elevates violent crime enables far stronger conclusions than were possible over a decade ago when the NRC Report was limited to analyzing data only through 2000 with the single tool of panel data evaluation.

The best available evidence using different statistical approaches—panel data regression and synthetic control—with varying strengths and shortcomings and with different model specifications all suggest that the net effect of state adoption of RTC laws is a substantial increase in violent crime.

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